

Explaining the dynamics of stagnation: an empirical examination of the North, Wallis and Weingast approach

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Explaining the dynamics of stagnation: An empirical examination of the North, Wallis and Weingast approach

Richard Bluhm, Denis de Crombrughe and Adam Szirmai

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This working paper is part of the research programme on 'Institutions, Governance and Long-term Economic Growth', a partnership between the French Development Agency (AFD) and the Maastricht Graduate School of Governance (Maastricht University – UNU-Merit). The research builds on the Institutional Profiles Database IPD, jointly developed by AFD and the French Ministry of the Economy since 2001.

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Explaining the dynamics of stagnation

*An empirical examination of the North, Wallis and Weingast approach**

Richard Bluhm[†] Denis de Crombrughe[‡] Adam Szirmai[§]

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WORKING PAPER

Abstract

This paper analyzes periods of economic stagnation in a panel of countries. We test if stagnation episodes are predicted by institutional factors and external/internal shocks, as is implied by recent theoretical contributions, and compare the impacts of these variables with those of traditional macroeconomic variables. We examine the determinants of stagnation episodes using multivariate dynamic linear models, fixed-effects logit models, and dynamic random effects probit models. In addition, we analyze whether the included variables have different impacts on the onset of a stagnation episode than on its continuation. We find that inflation, negative regime changes, real exchange rate undervaluation, financial openness, and trade openness explain the incidence of stagnation spells. Only in the case of trade openness, there is robust evidence of a differential impact; it reduces the probability of falling into a stagnation spell, but has a weaker effect within a spell. All models account for unobserved heterogeneity and exhibit a moderate degree of positive state-dependence.

Keywords: growth episodes, stagnation, institutions, dynamic panel data

JEL Classification: O11, O43, C25

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1 Introduction

Since the 1950s, most countries across the globe have experienced substantial welfare gains brought about by many years of positive economic growth and declining birth rates. However, while in the developed world these welfare gains are mainly the result of steady positive growth rates, GDP per capita growth in developing countries has been much more erratic and volatile. Most emerging economies have experienced periods of economic stagnation between positive growth spurts and for several countries the absence of sustained growth has proved to be a persistent phenomenon, often lasting for several years or even decades. Explaining why some countries experience more periods of stagnation than others may thus prove essential to the understanding of contemporary differences in levels of GDP per capita.

Rather than focusing on differences in average growth rates, recent research increasingly aims to unveil the specific characteristics of accelerating growth, stagnation, or decline. This paper addresses two research questions within this wider agenda. First, we ask if institutional characteristics and external/internal shocks determine the incidence of stagnation spells, and compare these effects to a set of macroeconomic variables. This question specifically tests the implications of a recent contribution by [North, Wallis and Weingast \(2009\)](#), who – among others – argue that economic collapses are affected by the ability of institutions to deal with various external and internal shocks challenging the prevailing distribution of economic rents and power. Second, we analyze if any of the included variables have a different impact on the onset of a stagnation episode than on its continuation. In other words, we examine if the factors affecting the probability of falling into stagnation are the same as those affecting the probability of continuing stagnation.

Most of the empirical literature on growth episodes uses static models to study factors that are correlated with the onset of a certain growth episode and, more recently, began to also examine factors associated with the duration of a given episode. Our contribution is to analyze stagnation spells as a dynamic problem, subject to state-dependence and interactions between the lagged state and the independent variables. Contrary to other studies, this approach allows the probability of stagnation to depend on whether a country was already in stagnation (state-dependence) and lets the data decide if the included variables have a different effect on the onset of a stagnation episode compared to its continuation, rather than separating these two *ex ante*. We estimate the dynamic models using fixed-effects linear probability models, GMM, fixed-effects logit and a dynamic random effects probit estimator proposed by [Wooldridge \(2005\)](#).

Our results indicate that political regime changes towards autocracy strongly affect the occurrence of stagnation episodes, while other proxies for institutions and shocks do not. Consequently, we find little support for theories along the lines of [North et al. \(2009\)](#). Instead, macroeconomic factors explain the onset of stagnation episodes rather well, but could themselves be endogenous to institutional characteristics. Higher inflation positively predicts subsequent stagnation episodes, while financial openness, trade openness and real exchange rate undervaluation are associated with less economic declines. Further, we find little evidence that the effects of these variables differ between the onset and continuation of stagnation spells. Only trade openness has robustly different effects. It has a large negative effect on the chances of falling into stagnation, but this effect vanishes and in some models switches signs during stagnation spells. In addition, we find that stagnation spells exhibit a moderate degree of state-dependence, which is consistent with other results in the literature on the duration of growth collapses.

The remainder of this paper is organized as follows. [Section 2](#) briefly reviews literature on institutions and growth, and discusses applications of the growth episodes approach. [Section 3](#) defines stagnation episodes and explores their correlations with GDP levels and institutions. [Section 4](#) describes the variables and data construction. [Section 5](#) outlines the empirical methods. [Section 6](#) discusses the results, and [Section 7](#) concludes.

2 Related literature

This paper primarily relates to two literatures. On the one hand, our research is guided by the findings of the theoretical and empirical literature on institutions and long-run growth. Specifically, a recent contribution by [North et al. \(2009\)](#) drew our attention to the importance of periods of per capita income stagnation in explaining long economic divergence (see the review in [Bluhm and Szirmai, 2012](#)). On the other hand, this paper is part of a burgeoning empirical literature on growth episodes and trend breaks.

An increasingly large body of literature in economics argues that differences in institutional characteristics are the key to understanding the discrepancy in long-run economic performances. While modern institutional theory has many antecedents, it builds on the hypothesis that well-developed property rights institutions could explain the historical rise of the West (e.g. [North and Thomas, 1973](#)). Since the 1990s, this literature has since been extended to view growth-promoting institutions less narrowly. Recent contributions argue, for example, that institutions for growth are multifaceted ([Rodrik, 2000](#)), interact with geography and inequality ([Engerman and Sokoloff, 1997](#)), develop semi-endogenously ([Greif, 2006](#)) and are deeply embedded in informal arrangements ([North et al., 2009](#); [Khan, 2010](#)).¹

In terms of econometric evidence, several papers have suggested that differences in institutions explain a large part – if not the largest part – of the cross-country variation in *levels* of GDP per capita.² However, many of these studies have also been criticized for their underlying assumptions (e.g. [Glaeser, La Porta, Lopez-de Silanes and Shleifer, 2004](#)) and do generally not establish a link between institutions and growth rates ([Meisel and Ould Aoudia, 2008](#); [de Crombrughe and Farla, 2011](#)). This is less puzzling than it may initially seem. A widely accepted fact is that institutional change is a slow process, often occurring over decades or even longer time horizons. For empirical studies this has a simple implication, more or less time-invariant characteristics are usually somewhat better-suited to explain differences in levels than variation in differences.

Potentially bridging this gap in theory, several authors have recently suggested that there is a link between various external/internal shocks to a country’s institutions and different growth outcomes. [North et al. \(2009\)](#), for example, identify two distinct regimes of stability, which they call ‘social orders’. Open access orders are economically and politically highly developed, experience relatively smooth patterns of economic growth, have active civil societies, many long-lived organizations, heavily formalized rules, and strong rule-of-law. Large segments of the population have access to political and economic organizations and activities. Limited access orders, on the contrary, are dominated by elites that exclude large parts of the population from access to economic and political organizations. The rents created in this process are then distributed among members

¹For a review of the debates see [Bluhm and Szirmai \(2012\)](#) and for an earlier survey see [Aron \(2000\)](#).

²This list of empirical studies investigating this issue is long and growing, but the seminal papers are [Knack and Keefer \(1995\)](#), [Hall and Jones \(1999\)](#), [Acemoglu, Johnson and Robinson \(2001, 2002\)](#) and [Rodrik, Subramanian and Trebbi \(2004\)](#).

of the ruling coalition. However, this distribution of rents among a select few has a deeper purpose, it serves to achieve a basic degree of social stability and control over violence. Limited access orders are characterized by volatile growth patterns, polities without broad democratic consent, few organizations, informal rules, weak and unequally enforced rule-of-law, insecure property rights and high levels of inequality.

North et al. (2009) suggest that limited access orders and open access orders deal differently with external or internal shocks to the distribution of rents and power among the ruling elites. Compared to open access orders, the institutions and rent-sharing agreements of limited access orders are inflexible and less able to contain conflict, thus causing a higher propensity towards growth collapses and stagnation. Khan (2010) provides a theory of rent sharing and highlights a growth-stability trade off in developing countries arising from institutionalized rent/power-sharing, but stresses the role of informal institutions and the relative size of the informal sector in the economy. Rodrik (1999) provides an early version of a theory of the relationship between institutions and growth collapses. He links negative growth experiences to terms of trade shocks, latent social conflict and the ability of institutions to contain conflict and absorb the destructive potential of such shocks. A key question for this paper is to what extent our analysis of stagnation episodes empirically supports these theories. Therefore, we hypothesize that (a) that institutional characteristics play an important role in explaining the onset of stagnation and (b) weak institutions prolong the incidence of stagnation spells.

As Pritchett (1998) pointed out, a substantial problem in traditional panel studies of growth rates is that they rely on identifying one average trend, while in reality growth is often erratic and may constitute very different growth regimes. This conjecture gave birth to a rapidly growing literature, which since has analyzed growth differentials (Rodrik, 1999), growth accelerations (Hausmann, Pritchett and Rodrik, 2005), switching among growth regimes (Jerzmanowski, 2006), the duration of growth collapses (Hausmann, Rodriguez and Wagner, 2008), start and stop growth (Jones and Olken, 2008), real income stagnation (Reddy and Minoiu, 2009) and the duration of upward trend breaks in growth (Berg, Ostry and Zettelmeyer, 2011).³

This paper relates most to the studies focusing on negative growth experiences. Rodrik (1999), for example, analyzes differences in growth rates between decades and differences before and after trend breaks to explain growth collapses. His contribution does not yet explore which factors relate to the incidence of these collapses, but analyzes the differences in growth rates between two growth regimes. He provides first evidence that growth collapses are linked to terms of trade shocks, latent conflict and the conflict management capacity of institutions. Hausmann et al. (2008) examine the onset and duration of growth collapses. They mainly find that weak export performance and high inflation coincide with the beginning of stagnation, but downturns also occur together with wars, sudden stops of capital inflows and political transitions. However, most of these factors have little influence of the duration of collapses, which only appears to be correlated with a measure of the flexibility of a country's export basket. Last, Reddy and Minoiu (2009) investigate stagnation spells and find that these are correlated with weak export performance, low investment, primary commodity exports and weak institutions.

The study of stagnation spells and other negative growth episodes is also related to the business cycle literature and the literature on economic crises. Although the focus of this

³Also considering studies with a regional focus extends this list substantially: for different parts of Africa see Pattillo et al. (2005), Arbache and Page (2007), and Imam and Salinas (2008), or for growth decelerations at middle income levels with a focus on China see Eichengreen, Park and Shin (2011).

paper is primarily on longer-run growth episodes and not on short-run fluctuations, these literatures provide relevant insights and hypotheses. For example, [Diebold, Rudebusch and Sichel \(1993\)](#) find that postwar business cycle contractions exhibit positive duration dependence⁴, while [Cerra and Saxena \(2008\)](#) show that after severe financial crises and wars growth does often not fully recover to its pre-crisis trend.

Many papers in the growth episodes literature use a methodology that can be summarized in two steps. First, a filter is applied to the data to identify single or multiple breaks in the time-series that is either rule-based or statistical. If the filter is rule-based, then often it includes a criterion defining the length of the spell, although this need not be the case. If the filter is econometric, then it may find more than one break in the data and thus lead to distinct episodes or growth regimes. Second, correlates with the *onset* of these episodes are examined by either testing differences in means of potentially correlated variables (across the onset), or by estimating probit models.

Interestingly, apart from [Jerzmanowski's \(2006\)](#) regime-switching models, none of these studies consider the incidence of a growth episode as a *dynamic* problem. Only [Hausmann et al. \(2008\)](#) and [Berg et al. \(2011\)](#) focus on the duration of collapses and thus use information of how long an episode lasts, but other studies generally disregard this information. We will address this method in more detail later, but essentially it assumes that the factors affecting the onset of an episode are not the same as those determining if an episode will continue. Further, most studies of growth episodes take very few measures to limit the endogeneity of the included regressors, which leaves unaddressed whether they are *causes of* or *caused by* the episode of interest.

3 Growth episodes and long-run growth

3.1 Defining the growth episodes

Our classification of growth episodes is closely related to the contribution of [Hausmann et al. \(2008\)](#) and to their definition of what constitutes growth collapses. We modify and extend their approach in two ways. First, we begin with their definition of a growth collapse, but find that the term stagnation episode more adequately describes the underlying phenomenon. Second, based on our definition of a stagnation episode, we then derive an entire classification of growth experiences.

We define a *stagnation episode* (or stagnation spell) as an event that begins with a contraction of GDP per capita (Y_{it}) in country i in year t and lasts until GDP per capita is again at or above its pre-episode level. A stagnation episode begins if and only if the level of GDP per capita in the year before the candidate year t is also at the maximum level within the time-series of the country. Defined formally, a stagnation episode begins when $Y_{it} < Y_{i,t-1}$ and $Y_{i,t-1} \geq \max_{x=t_0}^{t-1} Y_{i,x}$, and lasts until $Y_{i,t+p} \geq Y_{i,t-1}$, where $p \geq 1$. Conversely, we define all years when a country is not stagnating as *expansion episodes*. In other words, an expansion begins the first year a country has left or not yet experienced a stagnation spell and lasts until the beginning of the next stagnation.

Apart from being very simple, these definitions have many desirable properties. In theory, a completed stagnation episode has a net effect of zero on the level of GDP per capita, since it includes both the downturn and the associated recovery. Similarly,

⁴An event exhibits positive duration dependence if it is more likely to end the longer it lasts (increasing hazard) and negative duration dependence if it is more likely to continue (declining hazard). Hence, positive duration dependence corresponds to negative state-dependence and vice versa.

defining expansions as the mirror image of stagnation episodes implies that the effect of an expansion episode on the level of GDP per capita is always positive. We explicitly exclude growth that is merely restoring what was lost in past crises, as this growth does not account for long-run increases in GDP per capita. Some commonly used filters, such as Hausmann et al.’s (2005) growth accelerations filter, do not explicitly make this distinction between recoveries and expansions (or accelerations), and thus potentially identify episodes that are at least in part recoveries.

Our filter is “time free”, in the sense that it does not impose a maximum length onto the episodes. Since we use annual data, any episode has a minimum duration of one year but can actually last for the entire length of the sampled period (1951–2007). Based on this definition, we can identify long stagnation episodes that may include recurring short-run recessions with incomplete recoveries – incomplete in the sense of not meeting the criterion of exceeding the maximum level of GDP per capita prior to the crisis. These stagnation episodes thus exclude many business cycle fluctuations.⁵

For a better understanding of the dynamics within these two broader episodes, we further differentiate each of them into two sub-spells. In the case of stagnation episodes, we distinguish between *crises*, lasting from the beginning of the stagnation episode to the trough, and *recoveries*, lasting from the year after the trough until the end of the stagnation spell. We define the trough to occur at the minimum level of output occurring during a stagnation episode. In the case of expansions, we distinguish between *moderate expansions* with an average growth rate less or equal than 5% per annum and *rapid expansions* with an average growth rate greater than 5% per annum.⁶

[Figure 1 about here]

Figure 1 illustrates how our filter works graphically as applied to Angola and France. This example is typical for the different growth experiences of developed and developing countries and shows that the filter works reasonably well in identifying the episodes of interest. While Angola has had many years of positive growth throughout the sample period, we find only three short expansion spells of which only the last is a rapid expansion. Instead, most of the time, Angola was in one protracted stagnation episode lasting from 1975 until the end of 2004, which included significant volatility in between. On the contrary, the French economy grew steadily since 1951 and is characterized by protracted periods of moderate expansion, which are only temporarily interrupted by short stagnation spells. Given these two stylized cases, the incidence of stagnation spells may thus explain a large part of the difference in long-run levels of GDP per capita.

Figure 1 also highlights that some stagnation spells only consist of a crisis without an associated recovery, which arises due to a technical issue. Since we rely on annual data, we are not able to distinguish a recovery from an expansion year if growth in the recovery year immediately after the trough is rapid enough to put GDP per capita above its pre-stagnation episode maximum. In order to not overestimate the length of stagnation episodes, we consider such years as part of expansions and not part of recoveries. For this reason, some crisis episodes are immediately followed by an expansion, without an

⁵This is the main difference with business cycle analysis. We are not interested in fluctuations around a trend, but negative deviations from the preceding trend and cases in which growth does not fit the business cycle perspective.

⁶More precisely, we measure the growth rate across an expansion as: $\bar{g}_{t,t+q} = q^{-1}[\ln Y_{i,t-1+q} - \ln Y_{it}]$, where q is the duration of the expansion. We classify an episode as rapid if $\bar{g}_{t,t+q} > 0.05$, and slow to moderate if $\bar{g}_{t,t+q} \leq 0.05$.

intervening recovery.⁷ This decision should have little influence on our estimates in the empirical section of this paper, as we are primarily concerned with explaining the occurrence of stagnation episodes and not with their duration.

3.2 Growth profiles

While the main focus of this paper is on the dynamics of moving in and out of stagnation spells, we first take a more detailed look at questions relating to the distribution of growth episodes across countries and time. Does the occurrence of stagnation episodes explain large parts of the difference in GDP per capita among the developed and developing world today? Or similarly, as [North et al. \(2009\)](#) and others have suggested, do low income countries grow rapidly very often but lose the benefits because fast growth is offset by a higher propensity to experience collapses? For example, a key finding of [Hausmann et al. \(2005\)](#) is that growth is easy to ignite even in low-income countries but more difficult to sustain thereafter. Using the previously defined growth episodes, [Table 1](#) addresses these issues in more detail.

[[Table 1](#) about here]

We report two panels in [Table 1](#). The upper panel groups the relative incidence of each type of growth episode from 1951 to 2007 by quartiles of GDP per capita in 2007. The lower panel uses income groups in 1960 for comparison. Interestingly, the picture emerging from these two classifications is very different. When we group the distribution of growth episodes by end-of-sample income classes, we find that low income countries spend most of their time in stagnation, upper middle income countries almost half the time and high income countries only about a quarter. In other words, this suggests that the different propensity to experience stagnation spells is closely linked to development outcomes today. Further, using the finer classification of four distinct growth episodes, we find that a high proportion of crises at low and lower middle income levels are driving this relationship. However, once we exclude recoveries from the positive growth experiences, there is little indication that lower income countries grow rapidly very often or possibly even more often than high income countries. In fact, the opposite seems to be the case. While countries in the lowest income group spend relatively more of their expansions growing rapidly ($10.21/22.12 \approx 46.15\%$), higher income countries spend more time growing rapidly in total. Even in absolute terms, out of all the rapid expansions in the sample only 14.58% accrue to the lowest income group, 16.90% to the lower middle income group, but 33.98% to the upper middle group and 34.54% to the highest income group in 2007. [Table 1](#) confirms the finding of [North et al. \(2009\)](#) that presently poor countries have experienced less years of positive growth than rich countries. However, it contradicts their assertion that once poor countries grow, they do so more rapidly than their rich counterparts. The main reason for this difference is the exclusion of recovery years from years of positive growth.

⁷This complication also in part explains why we do not use the disaggregate definitions as a basis for the empirical analysis. For unfinished stagnation episodes, there is considerable uncertainty if the currently observed trough (beginning of the recovery) will actually remain the trough of the stagnation episode if we were to observe it until its end. In other words, both the length of the crisis period and the beginning of the recovery may be subject to change when data on more recent years come available. However, for stagnation episodes as a whole there is no ambiguity towards their beginning, therefore they do not suffer from this problem.

Nevertheless, the findings in [Table 1](#) could be driven by a tautology. The income level at the end of the sample is, by definition, affected by the number and length of stagnation episodes a country experienced during the sample period. The bottom panel of [Table 1](#) circumvents this problem by grouping the episodes across income classes in 1960, which is closer to the beginning of the sample.⁸ Some of the results are similar to the top panel, but others are radically different. The similarities are that rich countries spend much less of their time in crisis and much more of their time in expansions. However, the distinction between low and middle income classes almost completely disappears and the only pronounced differences that remain are between high income countries and the rest. High income countries in 1960 spent about 30% of their time in stagnation from 1951 to 2007, while middle and low income countries stagnated more than half of the time. Similarly, the incidence of crises no longer declines almost linearly with higher income groups but remains at about a third for the lowest two income group, slightly below a third for the upper middle income group and below 20% for the highest income group. Further, the findings of [Hausmann et al. \(2005\)](#) receive more support when we use the beginning-of-sample income groups. Lower and middle income countries spend relatively more time in rapid expansions than high income countries, but this difference is not very large and most pronounced only for lower middle income countries. If we examine the distribution of all rapid growth spells across income groups, we find that 24.55% occurred in low income countries, about 29% in lower middle income countries, 25.19% in upper middle income countries and 21.55% in high income countries.

In general, these results suggest that even if we net out the growth effects of recoveries it remains true that poorer countries can grow rapidly, although whether they do so relatively more often than their high income counterparts depends on the classification. That this effect is only weak can be linked to the related convergence debate. Although neoclassical growth theory predicts that low income countries catch up rapidly with the rest, absolute convergence is not taking place. The growth profiles presented here support the view that this absence of catching-up may be driven by the repeated and persistent occurrence of stagnation spells, rather than the inability of poorer countries to grow rapidly. They also are consistent with the view that some developing countries can catch up through accelerated growth, while many other countries fail to do so.

As we discussed, [North et al. \(2009\)](#) suggest that one plausible explanation for the lack of generalized convergence among economies is that the institutions in lower income countries deal less well with rapid change and various external and internal shocks. To recapitulate, open access orders are based on a system of impersonal rule, high formalization and open participation. Open participation and no barriers to organization together ensure that open access institutions adapt easily to changing circumstances and challenges. In contrast, limited access orders which characterize most of the developing world today are governed by ruling coalitions that rely on personal/informal networks and explicit rent-sharing agreements to contain conflict and ensure stability. As a result, their institutions are less adaptive, less able to adjust to shocks and these countries are more prone to economic crises and stagnation.

[Table 2](#) links the conjecture of [North et al. \(2009\)](#) and similar theories to the approach developed in this paper by cross-tabulating the different growth episodes with two indices of institutional quality. The data on institutions is derived from a paper by [de Crombrugghe and Farla \(2011\)](#), who aggregate a large number of indicators from the *Institutional Profiles Database* (IPD) 2009 using principal component analysis. They

⁸We chose 1960 as it allows us to classify 101 countries. In 1950, we can only classify 51 countries.

derive two principal components from the data and interpret their meaning. The first principal component describes the *institutional formalization of regulations*, while the second principal component measures the *degree of control and intervention by the state*.⁹ Similarly to the income classification used before, we group the scores on each component into quartiles ranked from low to high. The upper panel in Table 2 shows the results for the first component and the lower panel the results for the second.

[Table 2 about here]

There is a moderate negative correlation between the index measuring the institutional formalization of regulations and the incidence of stagnation episodes ($\rho \approx -0.5$ in 2007). The countries belonging to the highest quartile on this index are in stagnation less than 25% of the sample period, while those ranked in the lowest quartile stagnate almost 70% of the time. In many ways these results resemble those using income groups in 2007. For example, fast expansions occur relatively most often in the upper middle quartile and crises occur gradually less often at higher quartiles of the index. In line with the theory, this suggests that higher *institutional formalization of regulations* leads to less stagnation spells and increasingly steady growth. However, the resemblance between the tabulations using income quartiles in 2007 and the tabulations with the formalization index is to a large extent owed to the strong correlation ($\rho \approx 0.8$) between the log of GDP per capita in 2007 and the first principal component from the IPD data. In other words, while Table 2 suggests a strong role for formal institutions in shaping growth profiles, it does not reveal if this effect can be attributed solely to formal institutions. Apart from addressing causality issues, we would prefer to classify countries by beginning of period institutions, but are constrained by the available data.

The bottom panel of Table 2 gives a more differentiated picture. The second principal component is negatively correlated with stagnation spells. This index can be interpreted to represent the degree of the state’s involvement in the private economy but also its degree of authoritarianism. The lowest incidence of stagnation spells (31.83%) occurs within the group of countries scoring in the lower middle quartile of the index, whereas countries in the highest quartile stagnated during nearly 70% of the sample period. As de Crombrughe and Farla (2011, p. 17) point out “Western European countries, the USA, Canada, and Australia are at neither extreme of the [index]”, which suggests that very low scores represent weak states and very high scores represent mostly authoritarian regimes. This explains why the most stable growth profile is located in the lower middle quartile of the index and not at either end of the spectrum.

In sum, this brief overview of different growth episodes from 1951 to 2007 highlights two points. First, the incidence of stagnation spells is much higher in lower and middle income countries than in high income countries. Second, institutions and especially the degree of formalization of rules and regulations could be a key driver behind less steady growth and more crises/stagnation spells, but this effect requires further analysis.

4 Variables and data

In this section we briefly outline the construction of the panel dataset used in the ensuing analysis. The dependent variable is a binary indicator that is *unity* if country

⁹For more details on the construction of the indices see the original paper by de Crombrughe and Farla (2011). The Institutional Profiles Database 2009 and earlier waves of the expert survey are publicly accessible at www.afd.fr/home/recherche/bases-ipd.

i is experiencing a stagnation episode in year t and *zero* otherwise, where stagnation spells are defined according to [Section 3.1](#). The GDP per capita data is from the *Penn World Tables* 6.3 ([Heston et al., 2009](#)), which holds data for 189 countries and territories from 1950 to 2007.¹⁰ We identify a total of 578 stagnation episodes, or a total of 3,276 country-years of stagnation. The independent variables we include broadly belong to two categories: macroeconomic indicators and variables describing political institutions as well as external/internal shocks to these institutions. We elaborate on these below, but [Table A.1](#) in the Appendix also provides an overview of all independent variables.

Macroeconomic variables: We include a range of proxies for factors that are typically associated with sound macroeconomic management. Most of these variables have been found to significantly affect growth performance in traditional panel studies using annual, 5-year or 10-year growth rates. Hence, it is interesting to investigate to what extent our approach can confirm such results and how these variables fare compared to political/institutional factors in predicting the incidence of stagnation spells. Further, our research design allows us to test if these variables have a differential impact on onset versus the continuation of stagnation episodes.

In order to control for the level of development, we include the log of GDP per capita ($\text{Log } GDP/c_{(t-1)}$) in nearly all models. Its expected effect is negative, as richer countries tend to experience shorter and relatively fewer stagnation spells. Controlling for GDP also serves a practical purpose. As indicated in the previous section, indices measuring the quality of institutions and GDP are strongly correlated, so that including both prevents us from erroneously attributing effects of the one to the other.

Maintaining price stability is widely seen as the core task of central banks and emphasized by a variety of literatures. For example, a recent paper investigating positive growth spells by [Berg et al. \(2011\)](#) finds that high inflation reduces the length of fast growth spells. We expect high inflation to be positively correlated with the onset of stagnation spells. However, within a stagnation spell the role of inflation is harder to pinpoint as – together with the exchange rate – it is instrumental in bringing about internal devaluation and regaining international competitiveness. Our measure of inflation is 100 times the log of 1 plus the annual inflation rate. This measure is close to the actual inflation rate when that rate is small but also reduces the influence of larger values (e.g. rare periods of hyperinflation). The annual inflation data is from the IMF’s *International Financial Statistics* (IFS) appended with data from the *World Development Indicators* (WDI) whenever the former is missing.

In addition to domestic prices, we also measure if the exchange rate is overvalued or undervalued in real terms. While traditional growth models do not attribute a significant role to the real exchange rate, recent research finds that depreciations are beneficial for growth accelerations ([Hausmann et al., 2005](#)) and stimulate growth in general ([Rodrik, 2008](#)). This positive effect may operate through many channels, but is most commonly linked to export-led growth and the relative price of manufactured products. On the negative side, radical changes in the real exchange rate can also be evidence of excessive volatility and potential preludes to currency crises. If the former

¹⁰We do not use the entire PWT data for the analysis, but make two adjustments. First, we drop all countries with less than one million inhabitants (at the last recorded year) and, second, we also drop countries with fewer than 20 data points in the GDP per capita series – leaving 127 countries. These modifications mainly serve to weaken the influence of small (island) states on our estimates and maximize the group size in the resulting panel structure.

effects are sufficiently strong, exchange rate undervaluation could therefore weaken the likelihood of experiencing stagnation spells. To capture this effect, we follow [Rodrik \(2008\)](#) in constructing an index of exchange rate undervaluation ($RER\ Value_{(t-1)}$).¹¹ The index is centered at 0, with higher values indicating exchange rate undervaluation and lower values indicating overvaluation.

We include two measures of trade performance to, on the one hand, control for any overlap between the real exchange rate and export performance and, on the other hand, estimate the effects of trade on stagnation episodes directly. First, we measure the price of exports relative to imports, the terms of trade ($\Delta ToT_{(t-1)}$), as the annual log difference in the net barter terms of trade from the WDI and supplement this series with data from the IFS when there are gaps in the WDI series. Terms of trade growth, declines and shocks have been linked to growth collapses ([Rodrik, 1999](#); [Hausmann et al., 2008](#)), accelerations ([Hausmann et al., 2005](#)) and the premature end of fast growth spells ([Berg et al., 2011](#)). Second, we also estimate the effects of changes in the value of real merchandise exports ($\Delta Real\ Exports_{(t-1)}$), which we measure as the annual log difference in the exports volume index from the WDI, appended with data from the IFS to extend coverage. Growth in real exports has been suggested to significantly reduce the probability of the onset of a stagnation spell ([Hausmann et al., 2008](#)). We examine if this is also the case in the presence of dynamics.

Further, the growth literature has identified *de jure* financial and trade openness as two key policy variables that positively influence growth outcomes. To account for the former, we include the Chinn-Ito index ([Chinn and Ito, 2006](#)) of financial openness ($Fin.\ Openness_{(t-1)}$). This index is the first principal component of the inverse of four variables measuring restrictions on external accounts based on the IMF's *Annual Report on Exchange Arrangements and Exchange Restrictions* (AREAER). To account for the latter, we use a dummy measure for economic liberalization ($Trade\ Openness_{(t-1)}$) developed by [Sachs and Warner \(1995\)](#) and extended by [Wacziarg and Welch \(2008\)](#). This indicator is coded as one in years a given country is completely open to trade and zero otherwise. While the index's authors have linked their respective measures to average growth rates, the growth episodes literature has found financial openness to precede growth accelerations ([Hausmann et al., 2005](#)) and trade liberalization to reduce the risk that a fast growth spell ends ([Berg et al., 2011](#)). Financial liberalization can lead to both increasing capital inflows and financial deepening but also enable capital flight and generally volatile capital flows. Hence, its expected sign and size is not clear *ex ante*. On the contrary, we expect trade openness to sizably reduce the probability of experiencing stagnation spells.

Last, we include a measure for income inequality after taxes and transfers ($Inequality_{(t-1)}$). Net income inequality is not only an economic variable but just as much influenced by a country's political institutions. Most of the growth episodes literature does not systematically analyze the role of inequality, with the exception of an early study by [Rodrik \(1999\)](#) and recent evidence of a negative effect on the length of positive growth spells ([Berg et al., 2011](#); [Berg and Ostry, 2011](#)). In panel studies of average growth rates the effect of inequality remains disputed.¹² However, parts of neo-institutional theory ([Engerman and Sokoloff, 1997](#)) and earlier work on the interaction of inequality

¹¹This index is based on the PWT 6.3 and its construction is described in more detail in [Table A.1](#).

¹²Most of this debate took place in the late 1990s and early 2000s, for examples of key studies see [Perotti \(1996\)](#), [Deininger and Squire \(1998\)](#), [Barro \(2000\)](#), and [Forbes \(2000\)](#). See also [Roodman \(2009\)](#) on the instability of the GMM estimates in [Forbes \(2000\)](#).

and growth collapses (Rodrik, 1999) suggest a negative sign for inequality, while early growth theory suggests that inequality rises alongside rapid development and falls again at higher income levels (Kuznets, 1955). Our data for net income inequality is taken from Solt (2009), who appends, benchmarks, and standardizes the UNU-WIDER *World Income Inequality Database* (WIID) to improve cross-country comparability.

Institutional and ‘shock’ variables: This set of variables aims to capture both parts of the observable cross-country heterogeneity that can be attributed to institutions, as well as various shocks which require a response by the political/institutional structure. These shocks may be external or internal but have in common that they pose a challenge to the prevailing regime and/or a country’s institutional set up.

Cross-sectional studies of GDP levels find strong support that institutions explain large parts of long-run growth (Knack and Keefer, 1995; Hall and Jones, 1999; Acemoglu et al., 2001, 2002) and also provide evidence that growth-enhancing institutions (e.g. property rights or executive constraints) contribute to lower growth volatility (Acemoglu et al., 2003). In line with these findings and the theory of North et al. (2009), we expect that more open and democratically constrained¹³ institutions will reduce the probability of experiencing a stagnation spell. Our most straightforward measure of political institutions is the revised combined polity score ($Polity2_{(t-1)}$) from the Polity IV project (Marshall and Jaggers, 2010). This measure is the difference between a country’s score on the aggregate *institutionalized democracy* index and the score on the *institutionalized autocracy* index coded by the Polity IV project. It has a range from -10 (hereditary monarchy) to $+10$ (consolidated democracy). We rely on the Polity IV data, as the *Institutional Profiles Database* which we used in the preceding section and most other popular measures (e.g. WGI) have a very limited time dimension. For studies requiring time-series, the Polity IV data is unique as it provides indicators for every year, in many cases going back to 1800.

From Polity IV, we also derive two additional measures of political shocks that have previously been used in the literature on growth accelerations (e.g. Hausmann et al., 2005). Based on the variable REGTRANS, we code a dummy for positive regime changes ($Regchange_{(t-1)}$) as major positive changes of the political structure identified by at least a three-point improvement in the polity score. Conversely, we code negative regime changes ($Regchange_{-(t-1)}$) as a minimum three-point negative change in the polity score, including interregna and state failure.¹⁴ Intuitively, we expect negative regime changes to increase the probability of stagnation, while positive regime changes may have a stagnation deterring or a negligible effect.

We also include a dummy for the irregular exit of leaders ($Leader Exit_{(t-1)}$) based on *Archigos 2.9* (Goemans et al., 2009) as a proxy for internal shocks to a country’s political regime. This variable codes an irregular exit whenever a country’s major leader (president, chancellor, dictator and so forth) lost power by means violating established

¹³Polity IV scores countries on five indices capturing the openness of the political process and the constraints placed on individual actors. We interpret the data as measuring the degree of open institutions with narrow mandates. The components are ‘Competitiveness of Executive Recruitment’, ‘Openness of Executive Recruitment’, ‘Constraints on Chief Executive’, ‘Regulation of participation’, and ‘Competitiveness of Participation’.

¹⁴In the case of growth accelerations, Hausmann et al. (2005) originally found that these are preceded by negative and positive regime changes. Jong-A-Pin and De Haan (2008) point out that this conclusion was based on an error in coding the regime change variables to also include minor changes in the polity score. Following their correction, we do not code any type of regime change if REGTRANS=0.

rules and conventions. Such cases include, but are not limited to, the loss of power due to the removal by a foreign power, assassinations, ill health and domestic popular protest with foreign support. We focus on leader exit and not entry, as our aim is to link periods of stagnation to unexpected adverse events and not to their possible resolution. Some studies of growth accelerations have accounted for the sudden death of leaders in office (Hausmann et al., 2005; Jones and Olken, 2008) but usually not other types of exit.

In order to investigate the impact of large scale violence on stagnation spells, we include a dummy for the occurrence of $War/Conflict_{(t-1)}$ based on the UCDP/PRIO *Armed Conflict Dataset v.4-2011* (Gleditsch et al., 2002). We expect countries that are the location of an interstate war or large civil strife to be especially prone of falling into a stagnation spell. Our measure codes a country as a location of war if the UCDP/PRIO database records an intensity level of 2 or higher, corresponding to at least 1,000 battle-related deaths in a country-year, and if the country is recorded as a location of war. In the case of multiple conflicts, our measure relies on the conflict with the highest intensity.

5 Models and estimation techniques

Most extant studies of growth episodes use pooled probit or non-linear panel methods to study the *onset* of a certain type of growth episode, such as positive growth spells (e.g. accelerations or trend breaks) or negative growth spells (e.g. collapses or stagnation).¹⁵ For this purpose, these studies usually drop all but the first observation when countries are experiencing the episode of interest and estimate differences to all those periods when they do not. Only two recent papers concerned with the duration of growth collapses (Hausmann et al., 2008) or length of positive growth spells (Berg et al., 2011) use observations within the growth episode to estimate survival models. Apart from Jerzmanowski’s (2006) Markov-switching models, most studies do not model the occurrence of a growth spell as a dynamic process. However, genuine *state-dependence* may be a crucial feature of any episode in general and of stagnation spells in particular.

Dropping observations and disregarding information that can otherwise be used for inference is inefficient, especially if better estimation techniques can be applied utilizing the whole data. Moreover, in the case of growth episodes that already occur with low frequencies, doing so radically reduces the number of observations and introduces rare event bias (King and Zeng, 2001). In growth research, this practice was partially inspired by the political science literature on the onset of civil war. For example, Fearon and Laitin (2003) or Collier and Hoeffler (2004) argued that some factors may be associated differently with the onset of civil conflict in contrast to its continuation. Very recently, this assumption was challenged by Bleaney and Dimico (2011), who point out that theories of conflict rarely – if ever – indicate that some factors should have a different impact on the first occurrence of civil war than the entire war. Instead, they present a simple statistical framework to test this hypothesis and let the data decide which factors matter for onset and continuation, respectively. Many of their points carry over into the growth episodes literature without much need for qualification. Macroeconomics and neo-institutional economics provide little guidance as to why some factors should only

¹⁵Most studies drop within-spell observations and use probit or logit models to investigate the onset of various episodes (see Hausmann et al., 2005; Pattillo et al., 2005; Hausmann et al., 2008; Imam and Salinas, 2008; Eichengreen et al., 2011; Jong-A-Pin and De Haan, 2011), while others avoid this problem by mainly examining mean-shifts across an episode or onset of an episode (Arbache and Page, 2007; Jones and Olken, 2008).

relate to the beginning of, for example, stagnation spells rather than the entire spell. However, [Bleaney and Dimico \(2011\)](#) fail to recognize that when specifying dynamic non-linear models, special attention is required to address well-known econometric issues arising in such models, such as *spurious* state-dependence, endogeneity of the lagged state together with unobserved heterogeneity, the so-called initial conditions problem and the non-linearity of interaction effects.

Building on these criticisms, the remainder of this section outlines our empirical model, describes the research strategy in general, and discusses several linear and non-linear estimation techniques addressing the aforementioned complications. We then apply these techniques to our data in the next section.

5.1 Dynamic models

Our most basic dynamic model is:

$$y_{it} = \mathbf{1}[\mathbf{x}'_{it}\boldsymbol{\beta} + \rho y_{i,t-1} + \varepsilon_{it} > 0], \quad i = 1, \dots, n, \quad t = 2, \dots, T_i \quad (1)$$

where y_{it} is the binary outcome variable indicating whether or not a country i is in a stagnation spell in year t , \mathbf{x}'_{it} is the transpose of a vector of covariates, $y_{i,t-1}$ is an indicator if country i is in a stagnation spell in period $t - 1$, and ε_{it} is a country and time-specific error term that may include unobserved effects.

For ease of exposition, we write the general model as a single index model, leaving the functional form unspecified for now. This model can be interpreted as a first-order autoregressive process with covariates. Using the entire available sample constrains the parameters in $\boldsymbol{\beta}$ to be equal for stagnation spells as a whole. In order to allow the impact of the covariates to be different in a stagnation spell than during expansions and to include possible unobserved heterogeneity, we extend this model with interaction terms and expand the error:

$$y_{it} = \mathbf{1}[\mathbf{x}'_{it}\boldsymbol{\beta} + \rho y_{i,t-1} + \mathbf{x}'_{it}y_{i,t-1}\boldsymbol{\gamma} + \mu_i + \nu_{it} > 0], \quad i = 1, \dots, n, \quad t = 2, \dots, T_i \quad (2)$$

where, in the linear case, the elements of the parameter vector $\boldsymbol{\gamma}$ can be interpreted as slope shifts in the effects of the variables in \mathbf{x}_{it} if a country is in a stagnation spell (i.e. $y_{i,t-1} = 1$), while mean shifts are captured by ρ . The error term (ε_{it}) is now expanded into a time-invariant unobserved country-effect (μ_i) and a residual time-varying error (ν_{it}). For now, the unobserved effects may be fixed or random and no assumptions towards their distribution or correlation with the included variables are imposed. In most specifications, we also include 5-year time dummies in the vector \mathbf{x}_{it} but not its interaction with $y_{i,t-1}$.

This model allows us to test the hypothesis that a specific variable has a different effect within a stagnation spell than over the entire episode. For any given parameter γ_k that is an element of $\boldsymbol{\gamma}$, we simply test the null hypothesis that $\gamma_k = 0$. Similarly, we can also test the joint hypotheses that any subset of the vector $\boldsymbol{\gamma}$ or the entire vector is equal to zero. In the latter case, equation (2) reduces to (1).

As theory provides little guidance, our modeling strategy is to “let the data decide” which variables in \mathbf{x}_{it} require an interaction term with $y_{i,t-1}$ and which do not. We proceed in four steps. First, we specify a fully interacted model according to equation (2). Second, we test if those interaction terms that are individually insignificant at the 10% level are also jointly equal to zero. Third, based on the linear models, we specify a ‘parsimonious’ reference model that retains only those elements in $\boldsymbol{\gamma}$ that pass our

exclusion criteria and, fourth, we verify its results with non-linear techniques.

Estimating the model specified in (2) using linear or non-linear estimation methods usually requires more restrictive assumptions towards the underlying data-generating process, including the exogeneity of the included regressors and the structure of the unobserved heterogeneity. However, neglecting to account for these often results in biased estimators.¹⁶ The bias mainly arises from two sources. First, the lagged state ($y_{i,t-1}$) is fully endogenous and thus violates the usual assumptions made in simple static linear or non-linear models. Second, the unobserved effects are potentially correlated with the lagged state, leading to the issue of *spurious* versus *true* state-dependence and the related initial conditions problem. In the following, we discuss several linear and non-linear probability models that can account for some or all of these issues in more detail.

5.2 Linear probability models

The simplest and possibly most intuitive way to approach our problem is to specify a linear probability model (LPM) with country-fixed effects (FE). This approach is particularly attractive, as FE OLS requires no distributional assumptions about the unobserved effects and the OLS coefficients are usually a good approximation of marginal effects near the means of the variables. However, even in *static models*, the key problem with the LPM is that it does not take the bounded nature of the dependent variable into account. In other words, the LPM can predict probabilities *outside* the unit interval $[0, 1]$.

Further, the LPM is always subject to heteroscedasticity because the dependent variable is bernoulli distributed.¹⁷ The OLS estimate can be made robust to heteroscedasticity by using a sandwich estimator of the covariance matrix, but FGLS (feasible generalized-least squares) is better suited to account for the inherent heteroscedasticity and delivers more efficient estimates. FGLS is a two-step estimator, which weighs the observations by the inverse of an estimate of the standard deviation of the residuals derived from the LPM. However, only non-zero and non-negative weights are feasible, which implies that the predicted probabilities must be strictly within the unit interval ($0 < P(\hat{Y}_{it} = 1) < 1$), otherwise they require trimming to a value near the interval bounds.

In our specific application the LPM has two further shortcomings. First, we allow for unobserved effects (μ_i), but the LPM somewhat awkwardly constrains these effects to respect $\mathbf{x}'_{it}\boldsymbol{\beta} < \mu_i < 1 - \mathbf{x}'_{it}\boldsymbol{\beta}$.¹⁸ Second and more importantly, the inclusion of the lagged state means that the fixed-effects OLS estimate of ρ is downward biased (Nickell, 1981). While the second problem applies to samples with finite T , it is more severe in panels with a small time dimension and is decreasing in T . Our estimation sample usually has an average time dimension of approximately 18 years, therefore we do not expect this bias to be very large. To verify that our models are robust to this bias and compare the linear models to its non-linear counterparts with more restrictive distributional assumptions, we also estimate a dynamic linear probability (DLP) model using Generalized Methods

¹⁶As a case in point, Bleaney and Dimico (2011) specify a dynamic probit model but do not address endogeneity and unobserved effects, which is very likely to render their statistical tests unreliable.

¹⁷This implies that the expected variance of the binary outcome given \mathbf{x}_{it} will not be zero but $\mathbf{x}'_{it}\boldsymbol{\beta}(1 - \mathbf{x}'_{it}\boldsymbol{\beta})$, which can be shown easily. The variance of a bernoulli distributed variable is $p(1 - p)$, in our case $p = E[Y_{it}|\mathbf{x}_{it}] = \mathbf{x}'_{it}\boldsymbol{\beta}$, so $Var[Y_{it}|\mathbf{x}_{it}] = \mathbf{x}'_{it}\boldsymbol{\beta}(1 - \mathbf{x}'_{it}\boldsymbol{\beta})$, since $Var[\varepsilon|\mathbf{x}_{it}] = Var[y|\mathbf{x}_{it}]$ this indicates heteroscedasticity because the variance depends on \mathbf{x}_{it} .

¹⁸For an explanation of this issue in particular and a modern textbook treatment of the properties of the LPM in general see Wooldridge (2010).

of Moments (GMM) estimators. GMM techniques for dynamic panel models with small or fixed T and large N are designed to specifically deal with the endogeneity of the lagged state in the presence of unobserved heterogeneity and are being increasingly applied to dynamic linear probability models (e.g. Alessie et al., 2004).

Analogous to (2), the least-squares fixed-effects model we estimate is:

$$y_{it} = \mathbf{x}_{it}'\boldsymbol{\beta} + \rho y_{i,t-1} + \mathbf{x}_{it}'y_{i,t-1}\boldsymbol{\gamma} + \mu_i + \nu_{it}, \quad i = 1, \dots, n, \quad t = 2, \dots, T_i \quad (3)$$

First-differencing this equation removes the time-invariant unobserved effects (μ_i):

$$\Delta y_{it} = \Delta \mathbf{x}_{it}'\boldsymbol{\beta} + \rho \Delta y_{i,t-1} + \Delta \mathbf{x}_{it}'\Delta y_{i,t-1}\boldsymbol{\gamma} + \Delta \nu_{it} \quad (4)$$

However, the presence of the lagged state in (3) implies that the differences lagged state $\Delta y_{i,t-1}$ is still potentially correlated with $\Delta \nu_{it}$. To see this, suppose that country i experienced a large idiosyncratic shock in $t-1$ that causes $y_{i,t-1} = 1$, then the second term in $\Delta \nu_{it} = \nu_{it} - \nu_{i,t-1}$ and the first term in $\Delta y_{i,t-1} = y_{i,t-1} - y_{i,t-2}$ are also correlated.

To account for the endogeneity of $\Delta y_{i,t-1}$, Anderson and Hsiao (1982, AH) propose to use the second lags of y_{it} from the level equation (3) as instruments. This difference and levels estimator provides a consistent estimate of ρ . Similarly, we can use the same technique to instrument the interaction terms $\Delta \mathbf{x}_{it}'\Delta y_{i,t-1}$ with the second lag in levels to get consistent estimates of $\boldsymbol{\gamma}$.

Difference GMM generalizes the approach of AH to include all lags of order 2 and higher as instruments for the endogenous regressors (Holtz-Eakin et al., 1988; Arellano and Bond, 1991). The difference GMM estimator is based on the following moment conditions: $E[y_{i,t-s}\Delta \nu_{it}] = 0$ and $E[\mathbf{x}_{i,t-s}\Delta \nu_{it}] = 0$ for all $t \geq 2$ and $s \geq 2$. Further, it requires that the error terms are not serially correlated over time. The results obtained with this estimator are more efficient than the instrumental variables estimator of AH, as it uses all of the possible orthogonality conditions available in the sample. In practice, however, the relevance of longer lags as instruments for differences decreases quickly and leads to the ‘weak instruments problem’ (Roodman, 2009).

Arellano and Bover (1995) and Blundell and Bond (1998) extend the GMM approach further and argue that past levels may not always be good instruments for future differences leading to potentially biased and inefficient estimates, especially for random-walk like variables. Instead, they propose a system GMM estimator that instruments levels with lagged differences and estimates both equations simultaneously. This estimator is consistent and often provides more efficient estimates than difference GMM. The additional moment conditions exploited by this approach are: $E[\Delta y_{i,t-s}(\mu_i + \nu_{it})] = 0$ and $E[\Delta \mathbf{x}_{i,t-s}(\mu_i + \nu_{it})] = 0$ for all $t \geq 3$ and $s \geq 2$. In other words, the lagged differences should not be correlated with the unobserved effects. System GMM also requires the initial conditions (y_{i0}) to be in a stationary equilibrium, which is arguably an unnatural assumption to make in the analysis of stagnation episodes.

As shown by Roodman (2009), both difference and system GMM results are often unstable and strongly depend on the instrument matrix used. For this reason, we do not use it for model building but only apply system GMM to the previously derived ‘parsimonious’ specification to confirm whether the results remain within a reasonable range of the LPM estimates.

5.3 Non-linear probability models

So far, all the discussed techniques apply linear approximations to an inherently non-linear problem. Symmetric bounded functions, such as the logistic cumulative density or the cumulative normal distribution solve this basic problem of functional form misspecification and are commonly estimated using logit or probit models, respectively. However, modeling unobserved heterogeneity in non-linear binary choice models is more complicated than in the linear case, as within transformations or first differences do not eliminate the unobserved heterogeneity. Since the unobserved effects and the endogenous lagged state in dynamic probability models are correlated and non-removable by transformation, GMM-type instrumentation strategies are also not available.

In non-linear binary choice models, the assumptions made on the structure of the unobserved effects in practice also decides which type of model can be estimated. We apply two techniques: fixed-effects logit and dynamic random-effects probit. On the one hand, the fixed-effects logit estimator is less restrictive in its assumptions about the unobserved heterogeneity but similarly to the LPM with FE, it does not deal with the endogeneity of the lagged state. On the other hand, the correlated random-effects probit estimator requires explicit assumptions about the unobserved heterogeneity, but has been modified to account for the endogeneity of the lagged state, including solutions for the initial conditions problem (Heckman, 1981; Orme, 2001; Wooldridge, 2005).

Fixed-effects logit: The standard dummy variables fixed-effects logit model estimated by unconditional Maximum Likelihood (ML) runs into a statistical problem. Consider an adaptation of the simple model in equation (1), without the lagged state but including the unobserved effects:

$$P(y_{it} = 1 | \mathbf{x}_{it}) = F(\mathbf{x}_{it}'\boldsymbol{\beta} + \mu_i + \nu_{it}), \quad i = 1, \dots, n, \quad t = 1, \dots, T_i \quad (5)$$

where $F(\cdot)$ is the cumulative logistic distribution ($F(z) = e^z / (1 + e^z)$).

To estimate this equation, we need a consistent estimate over $t = 1, \dots, T_i$ for each of the unobserved effects μ_i because in ML estimation the log-likelihood function is maximized over the time-series of each group and then over groups. However, T_i is often small and as a result any inconsistency introduced there will create inconsistency in the estimate of $\boldsymbol{\beta}$ as it is a function of the estimated constants. This is the well-known *incidental parameters problem* which, for balanced panels, creates a bias in $\boldsymbol{\beta}$ in the order of $1/T$ (Greene, 2011). Similarly to the bias of the lagged state in the LPM, this bias is decreasing in T and hence, we do not expect it to be very large. Greene (2011, p. 621), for example, illustrates in Monte Carlo simulations using a sample of $N = 1000$ with 200 replications that the bias of $\boldsymbol{\beta}$ is only about 6.9% when $T = 20$, but as large as 100% if $T = 2$.

For the logit model, Chamberlain (1980) observed that there is a computational trick that allows consistent estimation of the parameter vector but not the constants by conditioning on the sum of observed outcomes *within* groups ($\sum_{t=1}^{T_i} y_{it}$). This conditional logit estimator result in a log-likelihood function where the incidental parameters μ_i drop out and which can be estimated using standard ML techniques. However, conditioning on the observed outcomes comes at a cost. Since groups in which y_{it} does not change over T_i provide no information for the likelihood (log of 1 is 0), they too drop out of the log-likelihood. In practice, if there's strong persistence such that $\bar{y}_i \approx 1$ or $\bar{y}_i \approx 0$, the number of observations used in the estimation may reduce by a lot – thus also changing the estimates. Likewise, time-invariant effects can no longer be included, as they would

also cancel out of the estimation equation. Further, since the μ_i are not identified but “conditioned out” and the partial effects depend on the expected value of the unobserved effects, the partial effects cannot be estimated either.

Given the expectation that the unconditional ML estimator is not too strongly biased and allows estimation of partial effects, we estimate both models and compare their results. For both the conditional and unconditional logit models, we specify the logit model implied by equation (2) with FE dummies in the case of unconditional logit and without in the case of conditional logit. However, neither of these two estimators nor other common logit approaches are able to account for the endogeneity of the lagged state – for this, we turn to random effects probit.

Random effects probit: While in the logit specification the unobserved effects can be removed by conditioning on the sum of the observed outcomes, this is not the case for the equivalent probit model. Even more restrictively, the standard random effects probit model assumes that the unobserved heterogeneity is not correlated with included regressors (strict exogeneity). Considering the probit variant of equation (1), then the presence of the lagged state ($y_{i,t-1}$) together with μ_i violates this assumption even if ρ is zero and invalidates the last equality (Wooldridge, 2010, p. 626):

$$P(y_{it} = 1 | y_{i,t-1}, \dots, y_{i0}, \mathbf{x}_{it}, \mu_i) = \Phi(\mathbf{x}'_{it}\boldsymbol{\beta} + \rho y_{i,t-1} + \mu_i + \nu_{it}) \neq P(y_{it} = 1 | \mathbf{x}_{it}) \quad (6)$$

where $\Phi(\cdot)$ is the cumulative normal distribution and y_{i0} are the initial conditions. This is the problem of *true* versus *spurious* state-dependence. The estimated effect of $y_{i,t-1}$ may depend on three sources: (1) serial correlation in the errors, (2) correlation with the unobserved effect and (3) true state-dependence (Greene, 2011, p. 729). In such a setting, the normal fixed or random effects estimation techniques do not provide consistent estimates of the parameter. Further, the outcome path may be severely influenced by the initial conditions (y_{i0}), which enter the unconditional likelihood function and prohibit integrating out the unobserved effects (μ_i). In short, to estimate dynamic random effects probit we are faced with two related problems, the assumption of strict exogeneity and the problem posed by a log-likelihood that includes the initial conditions.

A relaxation of the strict exogeneity assumption has been developed for static models by Mundlak (1978) and Chamberlain (1984), whose modification is known as *correlated random effects probit*. According to the Mundlak-Chamberlain approach, we can allow for correlation between \mathbf{x}_{it} and μ_i by restricting the distribution of unobserved effects to be related with means of the explanatory variables as follows:

$$\mu_i = \eta_0 + \bar{\mathbf{x}}'_i \boldsymbol{\eta}_2 + u_i \quad (7)$$

where the u_i are assumed to be i.i.d. and normally distributed. The vector $\bar{\mathbf{x}}_i$ consists of time-averages of the regressors \mathbf{x}_{it} . Further, this model implies that the composite error terms ($u_i + \nu_{it}$) are equicorrelated over any two different time periods: $\text{Corr}(u_i + \nu_{it}, u_i + \nu_{is}) = \sigma_u^2 / (\sigma_u^2 + \sigma_\nu^2)$ for any $t \neq s$, where σ_ν^2 is normalized to 1.

While this approach allows for correlated random effects, it does not address the issue of dynamics and the related initial conditions problem. Four solutions have been proposed to deal with this issue. One possibility is to assume that the initial conditions are completely exogenous and proceed with conditional maximum likelihood. However, this assumption is highly implausible as it requires that the unobserved effects (country effects) are independent of the initial state (Wooldridge, 2010, p. 626). A better but

complicated approach is to allow the initial conditions to be randomly distributed and approximate their distribution (Heckman, 1981). The third method is a related two-step bias correction procedure that also models the initial state (Orme, 2001). The last and latest proposal is to condition on the time-average covariates and the initial values in estimating the density $(y_{i1}, \dots, y_{iT_i})$ by specifying a distribution of the unobserved effects given the initial conditions (Wooldridge, 2005). While the latter approach does not specify a distribution of the initial conditions, it is especially attractive because it can be estimated using standard random effects software. Arulampalam and Stewart (2009) investigate the properties of these different estimators find that their performance is very similar for panels with a moderate time dimension and no estimator consistently outperforms the others in all simulations. For this reason, we apply the computationally undemanding Wooldridge estimator.

Concretely, Wooldridge (2005) proposes to specify the conditional distribution of the unobserved effects as $\mu_i|y_{i0}, \bar{\mathbf{x}}_i \sim \mathcal{N}(\eta_0 + \eta_1 y_{i0} + \bar{\mathbf{x}}_i' \boldsymbol{\eta}_2, \sigma_\eta^2)$, where $\bar{\mathbf{x}}_i$ are the time-averages as in Mundlak-Chamberlain. This results from the following parametric specification for the unobserved effects, where the u_i are normally distributed and independent of $(y_{i0}, \bar{\mathbf{x}}_i)$:

$$\mu_i = \eta_0 + \eta_1 y_{i0} + \bar{\mathbf{x}}_i' \boldsymbol{\eta}_2 + u_i \quad (8)$$

Substitution of (8) into the basic index model in equation (1) gives:

$$y_{it} = 1[\mathbf{x}_{it}' \boldsymbol{\beta} + \rho y_{i,t-1} + \eta_0 + \eta_1 y_{i0} + \bar{\mathbf{x}}_i' \boldsymbol{\eta}_2 + u_i + \nu_{it} > 0], \quad i = 1, \dots, n, \quad t = 2, \dots, T_i \quad (9)$$

Wooldridge (2005) also points out that this model can be extended by including interactions with the lagged-state and the corresponding interactions with the initial conditions. Given that we are precisely interested in testing these interactions, this is ideal. Hence, as a final model, we fit the Wooldridge version of equation (2):

$$\begin{aligned} \Pr(y_{it}|y_{i,t-1}, \mathbf{x}_{it}, y_{i0}, \bar{\mathbf{x}}_i, \mu_i) = \\ \Phi(\mathbf{x}_{it}' \boldsymbol{\beta} + \rho y_{i,t-1} + \mathbf{x}_{it}' y_{i,t-1} \boldsymbol{\gamma} + \eta_0 + \eta_1 y_{i0} + \mathbf{x}_{it}' y_{i0} \boldsymbol{\theta} + \bar{\mathbf{x}}_i' \boldsymbol{\eta}_2 + u_i) \end{aligned} \quad (10)$$

where \mathbf{x}_{it} is the vector of explanatory variables, $y_{i,t-1}$ is the lagged state, the vector $\boldsymbol{\gamma}$ allows for differential effects of the covariates within the state ($\mathbf{x}_{it} y_{i,t-1}$), η_1 measures the effect of the initial condition (y_{i0}), the vector $\boldsymbol{\theta}$ measures the effect of the covariates depending on the initial condition ($\mathbf{x}_{it}' y_{i0}$), $\bar{\mathbf{x}}_i$ are the time-means across units of the covariates. Estimation still proceeds over $i = 1, \dots, n$ and $t = 2, \dots, T_i$.

Using the law of iterated expectations, Wooldridge (2005) also shows that the average partial effects (APEs), which are not identified in the conditional logit model, are in fact identified when this method is applied.¹⁹ In other words, the dynamic random effects probit model addresses all three fundamental issues involved in our research problem. It correctly specifies the functional form of the dependent variable, relaxes the strict exogeneity assumption, and consistently estimates the APE of time-varying variables and the lagged-state in the presence of unobserved effects. Hence, we consider it the most appropriate for our data.

¹⁹In Appendix B, we further discuss the issue of partial effects of interaction terms in non-linear models, derive their standard errors using the Delta method, and summarize the argument from Wooldridge (2005) showing that the APEs of time-varying variables are indeed identified this model.

6 Estimation results

6.1 Linear models

Before presenting the results from the linear regressions, we first elaborate how we build the FGLS model in practice and highlight an interesting finding regarding the distribution of the predicted probabilities of stagnation. As discussed in the preceding section, the FGLS estimator requires an estimate of the conditional variance to account for the inherent heteroscedasticity. This variance estimate is typically based on the predictions of the linear probability model²⁰ and can easily be adjusted for clustering at the country level by averaging either the variance estimates or the predicted probabilities across countries. Figure 2 graphs the distribution of the (untrimmed) predicted probabilities using both approaches and illustrates why the latter approach is particularly attractive in our case. As expected, the LPM predicts probabilities outside of the unit interval (left panel), but when we average the probabilities across countries first, the estimates remain almost perfectly within the *open* unit interval (right panel). Hence, the second approach gets rid of many predictions that are otherwise problematic.²¹

[Figure 2 about here]

Figure 2 also reveals the surprising result that the predicted probabilities are clearly bimodal, with the modes being located near the bounds of the unit interval.²² In other words, *the LPM predicts some countries to almost always experience a stagnation spell and others almost never*. While this is a qualitatively interesting result, it also indicates that non-linear models will provide better estimates of the partial effects than the linear approximation, which works best with unimodal probability distributions.

In Table 3 we report the results from variations of the linear probability model under different estimation assumptions. We compute the LPM using OLS with country fixed-effects and 5-year dummies in two ways. The first model is the fixed-effects model with standard errors clustered at the country level. The second model is similar, but we now allow for clustering over years instead countries, thus making the standard errors robust to common shocks in given year. Model three shows the FGLS estimator with clustering at the country-level. Model four is the parsimonious FGLS specification discarding those interaction terms that are individually and jointly insignificant. Model five and six re-estimate model four using the system GMM approach with different instruments sets.

[Table 3 about here]

The bottom panel of Table 3 reports four rows (labeled “joint:”) containing the p-values of joint hypothesis tests. The first row tests the hypothesis that the country fixed-effects are zero, which is firmly rejected in all specifications that estimate country-effects. The second row tests the joint significance of the 5-year time dummies. Only

²⁰Computed as $\mathbf{v}_r = \hat{\mathbf{y}}(1 - \hat{\mathbf{y}})$, where $\hat{\mathbf{y}}$ is the vector of predictions.

²¹There are still many predictions (440) near the interval bounds, so we trim the mean probabilities according to $\hat{y}_i = .05$ if $\hat{y}_i^* \leq .5$ or $\hat{y}_i = .95$ if $\hat{y}_i^* \geq .95$, where \hat{y}_i^* refers to group-means of the untrimmed probabilities. This moves the estimates closer to the OLS results and ensures that these observations do not disproportionately influence the estimates. This process is somewhat arbitrary but justifiable. We trim the values to a conservative 5 percentage points distance from the interval bounds to lessen the influence of observations close to these bounds. For example, the weight of an observation trimmed to 0.05 is $1/\sqrt{0.05} \approx 4.47$, whereas trimming to 0.01 would imply $1/\sqrt{0.01} = 10$.

²²This is confirmed by the non-linear models which allow estimating a comparable distribution.

in the first model the 5-year dummies border on significance at the 10% level. In all other cases, we cannot reject the null that the time-effects are jointly zero. Rows three and four of the joint-tests are key to our model-building approach. Row three tests if the coefficients of the interactions with the lagged state (γ -vector) and the coefficient of the lagged state (ρ) are jointly zero; taken together these variables comprise Set I. In all models this hypothesis is rejected, indicating the presence of dynamics. In the row below, we test if the coefficients of those interactions with the lagged state that are individually insignificant at the 10% level in the current model (i.e. Set II) are also jointly equal to zero. In all specifications, the insignificant variables fail this joint-exclusion test.

In general, the fully-interacted models (1)–(3) in Table 3 give very similar results. For the variables without interactions, we find that inflation, financial openness, trade openness and negative regime changes significantly affect the probability of a stagnation spell. Furthermore, each of these variables enters with its expected sign. In model two, which allows for clustering per year, the standard error of inflation becomes slightly larger making it marginally insignificant at the 10%-level, while the standard error of the Polity2 index gets smaller and its coefficient becomes significant at the 5%-level. Considering the interactions with the lagged state, the coefficients of inflation, trade openness and negative regime changes are robustly different from zero in most models. Interestingly, in all three cases the interaction effect has the *opposite* sign to the non-interacted coefficient, indicating that the effects of these three variables are weaker within a stagnation spell. Model (1) also suggests that the dummies for irregular exits of leaders and wars/conflicts should have interaction terms, but these are not robust in the specification with clustering per year and the more efficient FGLS estimates.

Model (4) only retains those interactions that pass the exclusion tests, i.e. discarding all jointly and individually insignificant interactions terms from the previous specification. This model is our baseline and parsimonious specification which we later re-estimate with non-linear techniques. We still find evidence that inflation, trade openness and negative regime changes have a different impact within the spell than on the onset probabilities. However, the interaction effect of financial openness which is marginally significant in model three becomes insignificant once we remove the other redundant interactions.

The parsimonious specification in model (4) is also the easiest to interpret in terms of partial effects and economic significance. A one point increase in the inflation measure in $t - 1$ leads to a 0.5% higher probability of entering a stagnation spell if the country is not already stagnating. However, if the country was already in a stagnation spell in $t - 1$, then a unit increase only translates into a higher continuation probability of 0.02%. Since we measure inflation as $100 \cdot \ln(1 + gCPI)$, a unit increase on the index roughly corresponds to a 1 percentage point increase in inflation. Further, the effect of trade openness is very large, but also becomes considerably smaller once a stagnation episode has started. If the country is not in a stagnation spell in $t - 1$, being open to trade as measured by the Wacziarg-Welch dummy reduces the chance of stagnation the next year by 23.05%. Yet within a stagnation spell trade openness reduces the probability of continuation by only 2.2%. Negative regime changes in $t - 1$ increase the probability of a falling into a stagnation episode by an astonishing 27.4%, but this effect also vanishes within a spell where negative regime changes lower the probability of remaining in stagnation by -0.33%. Last, a one unit increase in the Chinn-Ito index of financial openness reduces the probability of stagnation by about 3.4% if the country was not stagnating the year before and by 1.1% within a stagnation spell. However, the coefficient of the interaction term is insignificant, suggesting that these two effects cannot be statistically distinguished. The

effects of each of these four variables are insignificant if a country is already in stagnation, suggesting that they matter for the onset of a stagnation spell but not for its continuation.

The model also indicates that there is considerable state-dependence, even though this effect is known to be downward-biased in linear fixed-effects models. The average partial effect of $y_{i,t-1}$ is about 0.293. In other words, if a country is already in a stagnation spell in $t - 1$, the probability of being in stagnation in year t is 29.3% higher compared to a country that is not in a stagnation spell in $t - 1$.

Interestingly, in the linear models we do not find any evidence that the log of GDP per capita, changes in the terms of trade, growth in real exports, real exchange rate undervaluation, inequality, institutions (Polity2), irregular leader exits, and war/conflicts in $t - 1$ have a significant and robust impact on either the onset or continuation of stagnation episodes. Further, the coefficients of GDP, inequality and wars/conflict do not have their expected sign, but their estimated effects are small and the confidence intervals around these point estimates are comparatively wide.

The system GMM specifications in models five and six mainly serve to assess if the parameter estimates remain similar once we account for the endogeneity of the lagged state and all interactions with the lagged state. For many variables this is not the case. Only the coefficient of inflation retains its approximate size and statistical significance, although the second model suggests a marginal effect that is almost ten times higher. The coefficient of negative regime changes stills point in the same direction, but its effect becomes implausibly large. Similarly, most other parameters appear rather unstable, which may be owed more to the inability of system GMM to identify their effects using the available internal instruments rather than any substantive reasons.

System GMM results strongly depend on the validity of the estimation assumptions. For this reason, we report several additional tests for the last two models in [Table 3](#). The Arellano-Bond tests (AB-Tests) examine the serial correlation structure of the errors for first-order autocorrelation (AR1) and second-order autocorrelation (AR2). Generally, the moment conditions are valid only if the AR2 test is rejected, while failure to reject the AR1 test is expected due to the presence of first-order dynamics. Both GMM specifications pass these tests. A more substantial problem in GMM estimation is overfitting through the use of too many instruments ([Roodman, 2009](#)), which [Table 3](#) reveals to be a primary concern in our case. We report the instrument count and the p-value of Hansen’s J-statistic, testing if the instruments are exogenous. The first GMM model uses the second and third lag of the endogenous variables as instruments in order to limit the total number of instruments. Nevertheless, this still results in a p-value for the J-statistic of unity. A perfect p-value can easily arise if the number of instruments is large relative to the group size and usually indicates that the test has very low power. We address this concern in the second GMM model by collapsing the instrument set to reduce the number of instruments to less than the number of countries.²³ We can still accept the hypothesis that the instruments are exogenous, but the parameters change considerably compared to the previous GMM model and the linear models.

The results suggest that applying GMM in our context is less than ideal. Five endogenous regressors and a moderate time-dimension quickly lead to instrument proliferation and therefore problems in identifying an instrument set that balances gains in efficiency with decreasing relevance of the instruments. As [Roodman \(2009\)](#) shows in simulations, the weak instruments problem can outweigh the benefits of applying GMM and lead to unstable or even irrelevant results. Further, system GMM also builds on

²³[Roodman \(2009\)](#) explains this technique in detail.

the assumption that the initial state is in equilibrium, which is untenable in our case. Accordingly, we place less emphasis on the GMM results and rely more on verifying the results from the preceding linear models with non-linear techniques.

In sum, this the linear models point towards several preliminary conclusions. First, in all specifications there is considerable evidence of unobserved heterogeneity at the country level, but less evidence in favor of time effects. Second, state-dependence plays a large role in determining whether a country experiences a stagnation episode or not. Third, we find that only inflation, financial openness, trade openness and negative regime changes have a statistically significant effect on the probability of stagnation, and fourth, all significant interaction terms with the lagged state point into the direction opposite of their non-interacted counterparts.

6.2 Non-linear models

As discussed in [Section 5](#), both the logit and probit estimators explicitly model the bounded nature of the dependent variable, while Wooldridge’s random effects probit estimator also provides a solution to the initial conditions problem and consistently estimates the effect of the lagged state and effects of interactions with the lagged state. In the following, we present the results from the logit and probit models, respectively.

Interpreting the effects of non-linear models additionally requires the computation of partial effects at the mean, at a representative value or as an average across the sample distribution. The derivative of a non-linear function with respect to any variable also depends on the value of the function at a particular point, which implies that the regression coefficients are not constant and not equivalent to the partial effects of these variables. However, in absence of other explicitly modeled non-linearities, the regression coefficients in probit and logit models usually indicate the sign and approximate significance of the corresponding partial effect, but are scaled in size. This relationship breaks down when interaction terms and other non-linear combinations are included. In fact, the partial effect of an interaction term can be different in *sign*, *size* and *significance* than the corresponding regression coefficient. Frequently, the partial effects of interactions even *change signs* across the distribution of predicted outcomes, leading some authors to conclude that these are best-examined graphically (e.g. [Ai and Norton, 2003](#); [Greene, 2010](#)). For the purposes of this paper, however, we are precisely interested in how much the partial effect of any interaction with the lagged state differs on average between the two relevant groups ($y_{i,t-1} = 1$ and $y_{i,t-1} = 0$) and in comparisons to the linear approximation. In order to reconcile this aim with the non-linearities of interaction terms, we compute both the average partial effects (APEs) and estimate the sample distribution of partial effects.²⁴

[[Table 4](#) about here]

[Table 4](#) presents the results using the fixed-effects logit estimators. We report three models and the corresponding APEs of the parsimonious model. As a reference, the first two columns show the fully interacted model estimated using conditional maximum likelihood, where the fixed-effects are not estimated but drop out. Model two is the conditional fixed-effects logit equivalent of the parsimonious linear model, and model three is the same model using dummy variables for the country-fixed effects instead of

²⁴We refer the reader to [Appendix B](#) for details on the calculation.

conditioning on the sum of observed outcomes per group. The last two columns report the APEs estimated on the basis of the previous fixed-effects specification. The APEs of interaction terms are reported similarly to coefficients of a linear model; that is, if $y_{i,t-1} = 0$, the APE is reported in the row of the non-interacted variable and, for $y_{i,t-1} = 1$, the total APE is the composite of the former and the APE of the interaction term.

Another complication in interpreting the results of the conditional logit model is that the APEs are not identified, for the simple reason that the individual country effects are not estimated. In order to approximate the APEs, we estimate the equivalent dummy variables fixed-effects logit model and compute the APEs based on its results. Models two and three using both conditional and unconditional logit show that the parameter estimates and the corresponding standard errors remain very similar. As expected, we find that the bias introduced by the incidental parameters problem is not large given the moderate time-dimension of the panel, which justifies our approach. However, applying conditional and unconditional logit substantially reduces the estimation sample to 1314 observations in 62 countries, for lack of within-group variation.

In terms of economic significance, many of the results in Table 4 resemble those of the linear models. We still find evidence that inflation, financial openness, trade liberalization and negative regime changes significantly affect the probability of stagnation. However, for all but the latter²⁵, the APEs of the interaction terms are *statistically insignificant* and only in the case of inflation and trade openness they still point in the opposite direction.

The APEs are also close to the linear approximation in terms of size. A unit increase in the inflation measure in $t - 1$ increases the probability of stagnation in t by 0.6% if the country was not stagnating in $t - 1$, but by 0.5% if the country was in stagnation in $t - 1$. Similarly, the APEs of trade openness and negative regime changes are still the largest effects in the model. However, the predicted average effect of trade openness if a country was stagnating in $t - 1$ is -10.57%²⁶, which is a larger negative effect than indicated by the linear model, and the APE of negative regime changes is predicted as 28.8% across the entire spell, because the interaction term is dropped. Further, the effect of financial openness is larger than in the linear case and the APE of the interaction term no longer has the opposite sign. The model predicts that a unit increase on the Chinn-Ito index decreases the probability of stagnation by 5.8% if the country was not already stagnating in the year before and by 6.99% if the country was in a stagnation spell in $t - 1$.

Further notable differences to the previous results are that now the effects of changes in the terms of trade, real exchange rate (RER) undervaluation and inequality are all significant at the 10% level and have increased substantially in absolute size. For example, a one standard deviation increase in the log difference of the terms of trade in $t - 1$ (which corresponds to an increase of about 12.6%) decreases the probability of stagnation by 1.5%, a one standard deviation change in the RER index towards more undervaluation decreases the probability of stagnation by 5.76% and one point increase in the gini index of inequality²⁷ raises the chances of stagnation by 0.7%. The effect of RER undervaluation is the largest in economic terms and borders on being significant at the 5%-level, the other two effects are comparatively small and only appear in parsimonious version of the logit models. Last, the degree of state-dependence identified by the logit model is somewhat

²⁵We cannot compute the partial effect for the interaction of negative regime changes with the lagged state, as this variable is dropped from the model.

²⁶This corresponds to the contrast when trade openness changes from 0 to 1 and remains at $y_{i,t-1} = 1$, which has a t-statistic of -1.55 and is thus borderline insignificant at the 10%-level.

²⁷The gini has a range from 0 to 100.

higher than in the linear approximation; a country that experienced a stagnation spell in $t - 1$ has a 32.1% higher probability of being in stagnation year t .

If were to solely rely on the APEs of the interaction effects to test the hypothesis that some variables have a differential impact on the onset of a stagnation spell than on its continuation, we would find little evidence in its support based on the logit models. Nevertheless, while the average interaction effects may not be different from zero, the interaction effects can still be individually significant for a substantial subset of observations. To examine the non-linearities of the interaction effects more closely, [Figure 3](#) graphs the distributions of partial effects for the interaction terms of inflation, financial openness and trade openness, as well as the associated p-values of a Wald-test of the null that the interaction effect at each particular observation is zero.

[[Figure 3](#) about here]

[Figure 3](#) clearly shows that all three interaction effects are strongly non-linear. In the case of inflation, we find that for some observations the partial effect is positive, while for most observations it is negative and insignificant. The effect is significant only at negative values for a very small fraction of the distribution (30 observations) and ranges from -0.95% to 0.39%, which is moderately large compared to an APE of 0.6% when $y_{i,t-1} = 0$. In all graphs, the predicted partial effects group into two families of curves with an S-shape. The curves at lower probabilities are the partial effects for observations where $y_{i,t-1} = 0$ (symbol: o) and the curves going across higher probabilities are the predicted effects for observations where $y_{i,t-1} = 1$ (symbol: x).

The interaction effect for financial openness is also S-shaped but sloping upwards and ranges from -6.9% to 7.6%. The partial effect is statistically significant for 14% of the predicted outcomes at both negative and positive values. However, similarly to inflation, the large range of insignificant negative and positive values supports the conclusion that the effect is not different from zero on average. On the contrary, for trade openness the evidence of a significant interaction is relatively strong. For most observations the partial effect of the interaction term is positive and very large, with an overall range from -3% to 30%. Furthermore, for about 32% of the observations in the sample the partial effect of the interaction term is individually significant at the 5%-level.

To summarize, the fixed-effects logit models generally confirm the findings of the linear models given a few refinements. Inflation, financial openness, trade openness and negative regime changes remain significant predictors of stagnation spells. Further, the models suggest that exchange rate undervaluation, changes in the terms of trade and inequality affect the probability of stagnation. However, the evidence in favor of interaction effects with the lagged state for any other variable than trade openness is weak; the models suggest that only trade openness has a different impact on the onset of a stagnation spell than its continuation.

[[Table 5](#) about here]

We now turn to the last and final set of estimates. [Table 5](#) presents the results the random effects probit estimators of the dynamic panel model. For comparison purposes, we first report the naïve pooled probit version of the fully-interacted dynamic model and then its random effects counterpart estimated according to the Wooldridge-approach. Model three is the Wooldridge estimator of the baseline parsimonious specification and

the last two columns report the APEs of the variables of interest and the corresponding bootstrapped standard errors.

We report several additional rows in Table 5 to display the regression results more succinctly. The row “ η_2 -estimated?” refers to whether the Chamberlain-Mundlack time-averages are included. As these not interesting by themselves, we do not report their coefficients and partial effects. Similarly, the row “ θ -estimated?” reports whether the interaction terms are included twice, once as an interaction with the lagged state and once as an interaction with the initial condition. We also do not report the parameter estimates of the latter, as these are primarily designed to account for the non-random nature of the initial conditions. Last, as before, all models still include 5-year time dummies to allow for common period-wise shocks, but their individual parameters are omitted in Table 5.

The comparison between the pooled probit and the random effects probit model clearly shows that the conclusions that would be derived from these two models are very different.²⁸ Several parameters exhibit sign changes and substantial changes in significance levels. More interestingly, even though the Wooldridge estimator of the dynamic model includes several additional terms and requires the assumption that the regressors are only correlated with the unobserved heterogeneity through their averages, the results are remarkably similar to the linear model and previous fixed-effects logit specification. As before, we interpret only the average partial effects of the parsimonious specification, but we are not able to also compute the partial effects at the observation-level as only the APEs across the entire distribution of the unobserved heterogeneity are identified in these models (see Wooldridge, 2005).

The APE of inflation is larger than in the previous models but remains highly significant. According to the Wooldridge estimator, a one unit increase in the inflation index in $t-1$ translates into 1.5% higher probability of a stagnation spell occurring in year t , if the country was not in stagnation in $t-1$. If the country experienced a stagnation spell in $t-1$, a one unit increase corresponds to a somewhat smaller 1.3% change in the probability. The APE of the interaction effect is insignificant and small, supporting the view that inflation increases the chances of stagnation, no matter if this occurs within or outside of a stagnation spell. Next, the APE of financial openness if $y_{i,t-1} = 0$ is similar to that estimated by the linear probability model, but not statistically significant in this model. A one unit increase in the Chinn-Ito index towards more openness reduces the probability of stagnation by about 3.3%. The APE of the interaction term is near zero (-0.1%) and insignificant, suggesting that there’s no difference between onset and continuation probabilities.

In the case of trade openness, the results differ substantially. A discrete change towards openness when $y_{i,t-1} = 0$ has an average partial effect of -8.7%, which is smaller than previous results and not statistically significant. However, the interaction term is still highly significant and has a very large effect on the predicted probability of stagnation (18.7%). Adding these two effects, we get the APE for the probability of continued stagnation if $y_{i,t-1} = 1$ and the country is open to trade, which is now *positive* and moderately large (10%). These results suggests that trade openness not only has a weaker effect within a stagnation spell, but that countries that are open to trade are more likely to continue stagnating than those that are closed, while countries that are closed are more likely to fall into a stagnation spell.

The confidence interval of the APE of negative regime changes when $y_{i,t-1} = 0$ widens

²⁸However, the coefficients of the probit and random effects probit models are scaled differently, so this comparison is more interesting in terms of relative sizes, signs and significance levels – where appropriate.

a bit, but the absolute size of the effect is still large and similar to that identified in the previous models. If a country was not experiencing a stagnation spell $t - 1$, the APE is 29.3%, but if it was stagnating in $t - 1$ and experiencing a negative regime change, the predicted probability of continuing stagnation is 25.9%. However, the APE of the interaction term is insignificant by a large margin and much smaller than the corresponding LPM estimate (-3.4% versus -27.7%).

The RE probit model confirms that RER undervaluation has a large and highly significant effect on the probability of stagnation. A one standard deviation change in the undervaluation index reduces the probability of both the onset and continuation of stagnation by 6%. We cannot corroborate the results from the logit model that changes in the terms of trade and inequality have a weakly significant effect on the probability of stagnation, but find weak additional evidence suggesting that institutions reduce the likelihood of stagnation. Finally, the Wooldridge estimator shows that the degree of state-dependence is underestimated by both the LPM and FE logit model. If a country experienced stagnation in the previous year, it is 36.5% more likely to remain stagnating.

In sum, the main results are robust to different specifications, assumptions towards the unobserved effects, and correlation between the heterogeneity and the country-effects. Most of the preferred specifications identify the lags of inflation and negative regime changes as strong predictors of stagnation episodes. Several models also identify trade openness and financial openness as relevant. The non-linear models mainly add three additional insights. First, there is less evidence of interaction effects between the lagged state and the included regressors than the LPM suggests. The interactions are highly non-linear and vary significantly across the sample, but only in the case of trade openness we find convincing evidence that its effect differs depending on whether the country was in stagnation in $t - 1$ or not. On the contrary, the evidence of interaction effects between the lagged state and inflation, financial openness or negative regime changes is weak. Second, the non-linear models show that the value of the real exchange rate matters for stagnation spells. An overvalued real exchange rate substantially raises the chances of stagnation, while undervaluation prevents it. Third, the level of state-dependence is still moderate, but larger than estimated by the linear models with fixed-effects.

We also find that lagging all included regressors by one year to assure that they are at least contemporaneously exogenous results in identifying very different factors than if the issue of endogeneity is left unaddressed. For example, [Hausmann et al. \(2008\)](#), who studied the onset of the stagnation spell we use in this paper, found that exports, inflation, wars and political transitions matter. Our models and measures are different than theirs, but we can only confirm their results for inflation and negative regime changes in particular, as opposed to political transitions in general. Our finding of moderate state-dependence, however, is potentially consistent with their results on declining hazards.

A key issue in this paper is testing if institutions and various shocks play a role in determining the onset or continuation of stagnation. Interestingly, apart from the large effect of negative regime changes, we find little evidence favoring institutional factors and proxies for shocks. On the contrary, traditional macroeconomics describes the occurrence of stagnation rather well. While these issues must certainly be explored further, the effect institutions have on periods of economic stagnation may not fully revealed in these models for two reasons. First, macroeconomic policy is likely to be endogenous to a country's institutions and the slow-moving effects of institutions may not be well-identified in panel studies such as this one. Second, the included measures may also characterize institutions insufficiently, leaving their true effect hidden in the unobserved heterogeneity.

7 Conclusion

This paper analyzed the incidence of stagnation episodes as a dynamic problem and asked if stagnation spells are determined by institutional factors, various shocks and macroeconomic factors. While the literature has examined the determinants of onset and duration of different episodes separately, we argue that there are few reasons to assume that these are different *ex ante*. We explicitly examine the hypothesis that the effects of variables on onset of stagnation and continuation are different. Building on a recent contribution by Hausmann et al. (2008), we define stagnation spells as episodes in which GDP per capita is below previously achieved levels. We then used fixed-effects linear models, GMM, fixed-effects logit and dynamic random effects probit to assess the role of institutions, shocks and economic factors, as well as to determine if any of these elements have a different effect on the onset and the continuation of a stagnation spell.

We identify several factors that predict the occurrence of stagnation spells. On the negative side, adverse regime changes have the single largest effect on the incidence of stagnation spells and higher inflation raises the chances of stagnation. More surprisingly, we find that real exchange rate undervaluation, financial openness and trade openness help reduce the chances of stagnation. Additionally, we find strong evidence indicating that trade openness primarily protects against falling into stagnation, but that this effect vanishes or possibly changes signs once a stagnation spell has begun. For all other variables there is no or only weak evidence of a differential impact on onset versus continuation, but these effects vary strongly across the distribution of outcomes (i.e. among individual countries). In most cases, we cannot confirm the hypothesis that there is a different impact on the first occurrence of stagnation than on continued stagnation.

Many of these results are consistent with findings of the previous literature on growth spells and panel studies of growth rates, i.e. the effect of inflation, financial openness, and the moderate degree of state-dependence (e.g. Hausmann et al., 2008; Reddy and Minoiu, 2009). Three findings stand out. Negative regime changes were previously linked to the onset of growth accelerations (Hausmann et al., 2005), but our findings reveal the more intuitive result that they strongly predict stagnation episodes. Trade openness has been shown to affect growth rates (Wacziarg and Welch, 2008), growth accelerations (Hausmann et al., 2005) and the duration of positive growth spells (Berg et al., 2011); we find it also prevents the onset of stagnation spells but affects their incidence non-linearly. Further, Rodrik (2008) shows that an undervalued exchange rate positively affects growth rates, to which we add that undervaluation also helps to avert stagnation spells.

Interestingly, we cannot confirm two central results of the previous literature. First, we find little evidence that, in addition to negative regime changes, institutions generally affect the incidence of stagnation spells. Second, contrary to previous evidence (Rodrik, 1999; Hausmann et al., 2008), we do not find that changes in real exports or terms of trade affect the chances of stagnation. Both effects are absent as we lag the independent variables to assure that they are contemporaneously exogenous. Especially, the former suggests that we cannot confirm the hypothesis that institutions and internal/external shocks determine the propensity of stagnation, as – *inter alia* – suggested by North et al. (2009). However, it is worth highlighting that this paper represents one way of looking at the data and of examining the underlying problem. Future work that uses more differentiated measures for institutions and additional techniques for dealing with endogeneity but continues to investigate differences between onset and continuation would add further insights to the results presented here.

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Appendix A

Figure 1 – Examples of growth episodes: Angola and France

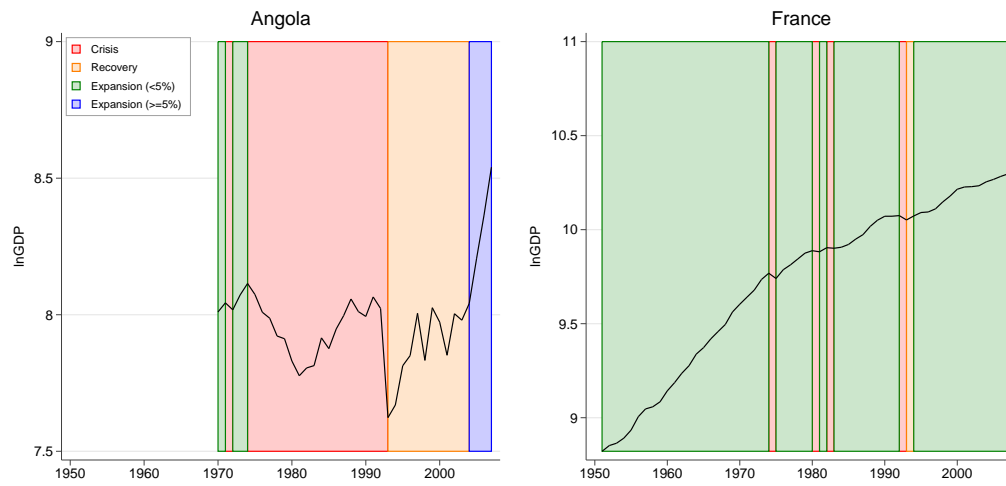


Figure 2 – Predicted Probabilities

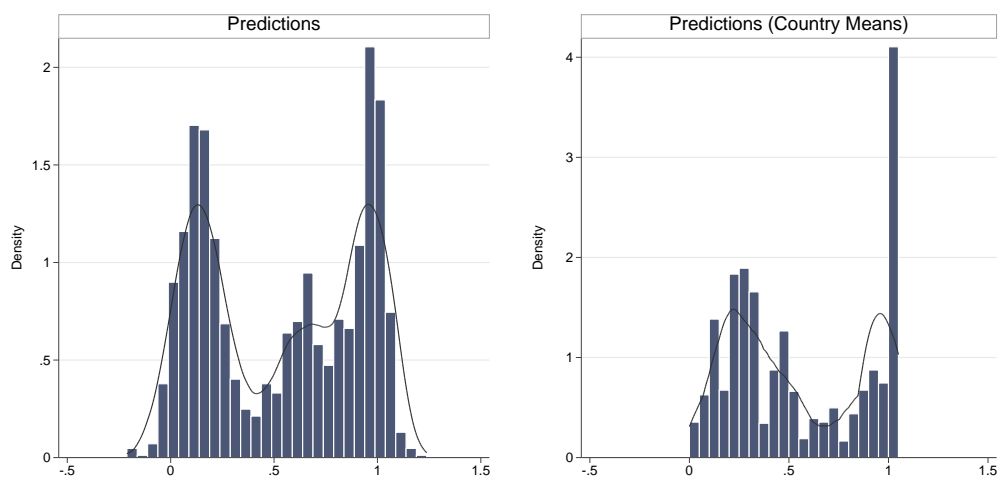


Figure 3 – Partial Effects of Interaction Terms in Logit Model

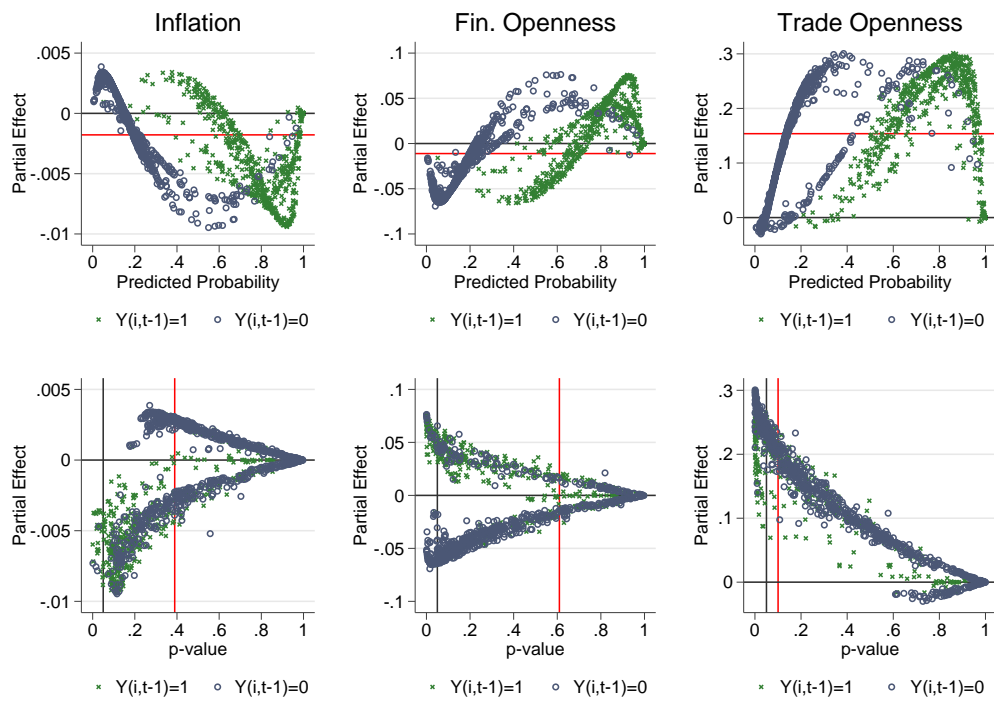


Table 1 – Growth episodes by income levels in 2007 and 1960

% Country-years in ...	Low	Low-Mid	Mid-High	High	Total
Panel A: <i>Income Level 2007</i>					
Expansion	22.12	41.33	54.97	73.14	48.31
Stagnation	77.88	58.67	45.03	26.86	51.69
Total	100.00	100.00	100.00	100.00	100.00
Expansion (above 5%)	10.21	11.52	23.18	22.66	16.99
Expansion (5% or less)	11.91	29.81	31.79	50.49	31.32
Crisis	49.90	30.63	23.81	17.42	30.18
Recovery	27.98	28.04	21.22	9.44	21.51
Total	100.00	100.00	100.00	100.00	100.00
Panel B: <i>Income Level 1960</i>					
Expansion	41.45	40.96	46.37	70.23	50.18
Stagnation	58.55	59.04	53.63	29.77	49.82
Total	100.00	100.00	100.00	100.00	100.00
Expansion (above 5%)	17.87	20.82	17.56	14.07	17.51
Expansion (5% or less)	23.59	20.14	28.81	56.16	32.67
Crisis	34.80	35.32	28.74	19.35	29.32
Recovery	23.74	23.72	24.89	10.42	20.50
Total	100.00	100.00	100.00	100.00	100.00

Notes: 51 countries in 1951, 127 countries in 2007, total number of observations 6,338 (Panel A) and 5,467 (Panel B), percentages calculated on the basis of all years between 1951 and 2007.

Source(s): based on Penn World Tables 6.3.

Table 2 – Growth episodes by institutional indicators

% Country-years in ...	Low	Low-Mid	Mid-High	High	Total
Panel A: <i>Formalization of regulations</i>					
Expansion	30.49	40.92	54.61	76.41	51.06
Stagnation	69.51	59.08	45.39	23.59	48.94
Total	100.00	100.00	100.00	100.00	100.00
Expansion (above 5%)	14.49	16.15	20.86	18.38	17.47
Expansion (5% or less)	16.00	24.78	33.75	58.03	33.60
Crisis	40.29	37.13	21.80	14.51	28.29
Recovery	29.22	21.95	23.59	9.08	20.65
Total	100.00	100.00	100.00	100.00	100.00
Panel B: <i>Control and intervention</i>					
Expansion	54.98	68.17	50.56	30.55	51.06
Stagnation	45.02	31.83	49.44	69.45	48.94
Total	100.00	100.00	100.00	100.00	100.00
Expansion (above 5%)	25.36	13.77	20.68	11.67	17.47
Expansion (5% or less)	29.62	54.40	29.87	18.88	33.60
Crisis	26.81	18.13	28.39	39.96	28.29
Recovery	18.21	13.70	21.05	29.49	20.65
Total	100.00	100.00	100.00	100.00	100.00

Notes: 47 countries in 1951, 107 countries in 2007, total number of observations 5,405 (Panel A and Panel B), percentages calculated on the basis of all years between 1951 and 2007.

Source(s): based on Penn World Tables 6.3 and [de Crombrughe and Farla \(2011\)](#).

Table 3 – Linear Models – Probability of Stagnation

Independent Variable	(1) LPM	S.E.	(2) LPM	S.E.	(3) FGLS	S.E.	(4) FGLS	S.E.	(5) GMM	S.E.	(6) GMM	S.E.
Log GDP/ $c_{(t-1)}$	0.052	0.073	0.052	0.098	0.061	0.073	0.040	0.065	0.001	0.015	-0.001	0.026
Inflation $_{(t-1)}$	0.005*	0.003	0.005	0.003	0.005**	0.002	0.005**	0.002	0.006*	0.004	0.049**	0.020
Δ ToT $_{(t-1)}$	-0.080	0.081	-0.080	0.076	-0.092	0.109	-0.036	0.054	-0.009	0.051	-0.104	0.063
Δ Real Exports $_{(t-1)}$	-0.136	0.169	-0.136	0.150	-0.080	0.143	-0.014	0.047	0.011	0.068	0.069	0.077
RER Value $_{(t-1)}$	-0.150*	0.088	-0.150	0.108	-0.083	0.075	-0.016	0.039	-0.094***	0.034	-0.062	0.072
Fin. Openness $_{(t-1)}$	-0.043***	0.016	-0.043**	0.018	-0.042**	0.017	-0.034**	0.016	0.001	0.019	0.090	0.058
Trade Openness $_{(t-1)}$	-0.236***	0.081	-0.236***	0.061	-0.233***	0.062	-0.231***	0.058	-0.073	0.121	-0.016	0.252
Inequality $_{(t-1)}$	-0.007	0.004	-0.007	0.004	-0.003	0.004	-0.002	0.003	0.005***	0.001	0.003	0.003
Polity2 $_{(t-1)}$	-0.011	0.007	-0.011**	0.005	-0.008*	0.005	-0.003	0.003	-0.004	0.004	-0.004	0.003
Regchange + $_{(t-1)}$	0.033	0.068	0.033	0.095	0.006	0.103	0.017	0.032	0.036	0.038	0.013	0.045
Regchange - $_{(t-1)}$	0.338*	0.187	0.338*	0.189	0.277*	0.149	0.274*	0.143	0.717**	0.320	1.451	1.037
Leader Exit $_{(t-1)}$	-0.071	0.053	-0.071	0.051	-0.045	0.054	0.004	0.028	-0.027	0.045	-0.028	0.042
War/Conflict $_{(t-1)}$	0.083	0.065	0.083	0.114	0.097	0.087	-0.005	0.042	-0.035	0.082	-0.121	0.154
Log GDP/ $c_{(t-1)} \cdot Y_{(t-1)}$	0.002	0.048	0.002	0.047	-0.039	0.042	-0.005**	0.002	-0.006	0.004	-0.049**	0.020
Inflation $_{(t-1)} \cdot Y_{(t-1)}$	-0.004*	0.003	-0.004	0.003	-0.005**	0.002	-0.005**	0.002				
Δ ToT $_{(t-1)} \cdot Y_{(t-1)}$	0.031	0.093	0.031	0.079	0.067	0.125						
Δ Real Exports $_{(t-1)} \cdot Y_{(t-1)}$	0.117	0.166	0.117	0.150	0.073	0.151						
RER Value $_{(t-1)} \cdot Y_{(t-1)}$	0.146	0.103	0.146	0.087	0.081	0.075						
Fin. Openness $_{(t-1)} \cdot Y_{(t-1)}$	0.021	0.019	0.021	0.017	0.034*	0.018	0.023	0.016	-0.004	0.027	-0.119	0.088
Trade Openness $_{(t-1)} \cdot Y_{(t-1)}$	0.186**	0.084	0.186**	0.078	0.207***	0.066	0.208***	0.059	0.018	0.134	-0.073	0.349
Inequality $_{(t-1)} \cdot Y_{(t-1)}$	0.001	0.003	0.001	0.004	0.001	0.003						
Polity2 $_{(t-1)} \cdot Y_{(t-1)}$	0.007	0.007	0.007	0.005	0.007	0.005						
Regchange + $_{(t-1)} \cdot Y_{(t-1)}$	-0.017	0.068	-0.017	0.098	0.011	0.108						
Regchange - $_{(t-1)} \cdot Y_{(t-1)}$	-0.354*	0.184	-0.354*	0.185	-0.280*	0.166	-0.277*	0.160	-0.827**	0.341	-2.041	1.910
Leader Exit $_{(t-1)} \cdot Y_{(t-1)}$	0.101*	0.060	0.101	0.068	0.059	0.060						
War/Conflict $_{(t-1)} \cdot Y_{(t-1)}$	-0.171**	0.081	-0.171	0.111	-0.126	0.096						
$Y_{(t-1)}$	0.158	0.473	0.158	0.492	0.486	0.423	0.222***	0.059	0.672***	0.150	1.135**	0.458
Constant	0.703	0.660	0.703	0.745	0.463	0.647	0.617	0.560	0.000	0.000	-0.303	0.410
Observations	1691		1691		1691		1691		1691		1691	
Country FE	YES		YES		YES		YES		-		-	
Clusters	country		year		country		country		country		country	
Joint: Country FE [p-values]	0.000		0.000		0.000		0.000		0.198		0.365	
Joint: 5-Year FE [p-values]	0.101		0.194		0.430		0.364		0.000		0.000	
Joint: Set I [p-values]	0.000		0.000		0.000		0.000		0.000		0.000	
Joint: Set II [p-values]	0.796		0.379		0.708		0.167		0.232		0.530	
Instruments									369		35	
AB-Test AR1									0.000		0.000	
AB-Test AR2									0.746		0.181	
Hansen's J									1.000		0.335	

*** p<0.01, ** p<0.05, * p<0.1

Table 4 – Logit Models – Probability of Stagnation

Independent Variable	(1)		(2)		(3)		(4)	
	Logit CML	S.E.	Logit CML	S.E.	Logit ML	S.E.	APEs	S.E.
Log GDP/c _(t-1)	0.417	0.707	0.430	0.695	0.430	0.738	0.058	0.099
Inflation _(t-1)	0.037	0.025	0.043	0.026	0.046	0.028	0.006*	0.004
Δ ToT _(t-1)	-0.556	0.571	-0.828*	0.479	-0.885*	0.516	-0.119*	0.070
Δ Real Exports _(t-1)	-0.784	1.163	-0.621	0.735	-0.681	0.795	-0.091	0.107
RER Value _(t-1)	-1.375**	0.567	-0.957*	0.503	-1.020*	0.533	-0.137*	0.071
Fin. Openness _(t-1)	-0.453***	0.121	-0.396***	0.110	-0.421***	0.118	-0.058***	0.016
Trade Openness _(t-1)	-1.441***	0.486	-1.424***	0.456	-1.527***	0.489	-0.259***	0.091
Inequality _(t-1)	-0.050	0.031	-0.052*	0.029	-0.054*	0.030	-0.007*	0.004
Polity2 _(t-1)	-0.066	0.044	-0.051	0.033	-0.054	0.036	-0.007	0.005
Regchange + _(t-1)	0.193	0.561	0.129	0.376	0.136	0.397	0.018	0.054
Regchange - _(t-1)	2.102**	0.900	1.945**	0.938	2.038**	0.991	0.288**	0.135
Leader Exit _(t-1)	-0.501	0.426	-0.060	0.317	-0.058	0.334	-0.007	0.044
War/Conflict _(t-1)	0.115	0.382	-0.428	0.342	-0.454	0.357	-0.059	0.045
Log GDP/c _(t-1) · Y _(t-1)	0.082	0.284						
Inflation _(t-1) · Y _(t-1)	-0.012	0.012	-0.019	0.015	-0.021	0.016	-0.002	0.002
Δ ToT _(t-1) · Y _(t-1)	-1.128	1.025						
Δ Real Exports _(t-1) · Y _(t-1)	0.290	1.244						
RER Value _(t-1) · Y _(t-1)	0.859	0.624						
Fin. Openness _(t-1) · Y _(t-1)	0.144	0.117	0.045	0.107	0.045	0.116	-0.011	0.022
Trade Openness _(t-1) · Y _(t-1)	0.947*	0.528	0.885*	0.487	0.939*	0.521	0.153	0.094
Inequality _(t-1) · Y _(t-1)	0.003	0.019						
Polity2 _(t-1) · Y _(t-1)	0.013	0.042						
Regchange + _(t-1) · Y _(t-1)	-0.164	0.620						
Leader Exit _(t-1) · Y _(t-1)	0.833*	0.506						
War/Conflict _(t-1) · Y _(t-1)	-1.051*	0.537						
Y _(t-1)	-0.004	2.813	1.189***	0.456	1.282***	0.487	0.321***	0.029
1970 - 1974 (b. 95-99)	-0.760	0.628	-0.756	0.622	-0.806	0.656		
1975 - 1979 (b. 95-99)	-1.059*	0.541	-1.097**	0.542	-1.182**	0.574		
1980 - 1984 (b. 95-99)	-0.248	0.469	-0.265	0.459	-0.302	0.486		
1985 - 1989 (b. 95-99)	-0.551	0.371	-0.544	0.354	-0.582	0.375		
1990 - 1994 (b. 95-99)	0.139	0.264	0.126	0.272	0.130	0.287		
2000 - 2004 (b. 95-99)	0.343	0.263	0.368	0.258	0.376	0.276		
Observations	1314		1314		1314		1314	
Country FE	YES		YES		YES		YES	
Clustered Errors [Country]	YES		YES		YES		YES	
Countries	62		62		62		62	
Log-pseudolikelihood	-465.864		-469.311		-555.547		-555.547	

*** p<0.01, ** p<0.05, * p<0.1

Notes: In all models Regchange +_(t-1) · Y_(t-1) is dropped and 10 observations not used, because != 0 predicts success perfectly. Standard errors of the APEs are computed using the delta method (see Appendix B).

Table 5 – Probit Models – Probability of Stagnation

Independent Variable	(1) Probit	S.E.	(2) Woold	S.E.	(3) Woold	S.E.	(4) APEs	S.E.
Log GDP/ $c_{(t-1)}$	0.027	0.103	0.862**	0.392	0.486	0.371	0.100	0.076
Inflation $_{(t-1)}$	0.017***	0.006	0.055***	0.014	0.060***	0.014	0.015***	0.003
Δ ToT $_{(t-1)}$	-0.328	0.300	-0.160	0.512	-0.278	0.351	-0.057	0.072
Δ Real Exports $_{(t-1)}$	-0.226	0.540	-0.094	0.677	-0.347	0.359	-0.071	0.074
RER Value $_{(t-1)}$	-0.719***	0.202	-1.164***	0.333	-0.607**	0.283	-0.124**	0.058
Fin. Openness $_{(t-1)}$	-0.056	0.039	-0.209**	0.083	-0.136	0.083	-0.033	0.020
Trade Openness $_{(t-1)}$	-0.567***	0.217	-0.415	0.268	-0.335	0.262	-0.087	0.072
Inequality $_{(t-1)}$	0.014**	0.007	-0.009	0.017	-0.021	0.016	-0.004	0.003
Polity2 $_{(t-1)}$	-0.014	0.013	-0.035	0.022	-0.031*	0.016	-0.006*	0.003
Regchange + $_{(t-1)}$	0.039	0.269	0.190	0.424	0.057	0.225	0.012	0.046
Regchange - $_{(t-1)}$	1.074*	0.557	1.216**	0.607	1.105*	0.589	0.293*	0.154
Leader Exit $_{(t-1)}$	-0.196	0.188	-0.058	0.252	-0.028	0.173	-0.006	0.035
War/Conflict $_{(t-1)}$	0.110	0.257	-0.087	0.398	-0.135	0.249	-0.028	0.051
Log GDP/ $c_{(t-1)} \cdot Y_{(t-1)}$	-0.259	0.182	-0.245	0.160				
Inflation $_{(t-1)} \cdot Y_{(t-1)}$	-0.008	0.006	-0.001	0.008	-0.008	0.008	-0.002	0.002
Δ ToT $_{(t-1)} \cdot Y_{(t-1)}$	-0.091	0.393	0.770	0.995				
Δ Real Exports $_{(t-1)} \cdot Y_{(t-1)}$	-0.435	0.574	0.429	0.776				
RER Value $_{(t-1)} \cdot Y_{(t-1)}$	0.635	0.390	0.427	0.317				
Fin. Openness $_{(t-1)} \cdot Y_{(t-1)}$	0.096	0.060	0.119	0.081	0.001	0.069	-0.001	0.017
Trade Openness $_{(t-1)} \cdot Y_{(t-1)}$	0.474	0.321	0.852***	0.276	0.720***	0.251	0.187***	0.066
Inequality $_{(t-1)} \cdot Y_{(t-1)}$	0.005	0.012	0.015	0.011				
Polity2 $_{(t-1)} \cdot Y_{(t-1)}$	0.008	0.022	0.022	0.021				
Regchange + $_{(t-1)} \cdot Y_{(t-1)}$	0.166	0.284	0.158	0.600				
Regchange - $_{(t-1)} \cdot Y_{(t-1)}$			2.960	1.917	2.851	1.186	-0.034	0.288
Leader Exit $_{(t-1)} \cdot Y_{(t-1)}$	0.290	0.259	0.393	0.307				
War/Conflict $_{(t-1)} \cdot Y_{(t-1)}$	-0.267	0.404	-0.754*	0.453				
Y_0			4.588**	2.064	1.521***	0.329	—	—
$Y_{(t-1)}$	3.407*	1.787	2.212	1.612	0.858***	0.242	0.365***	0.034
Constant	-1.287	1.105	-5.499**	2.316	-2.483	1.980	—	—
Observations	1586		1596		1596		1596	
γ -estimated?	NO		YES		YES		YES	
θ -estimated?	NO		YES		YES		YES	
5-Year FE	YES		YES		YES		YES	
Countries	90		90		90		90	
Pseudo-R ²	0.423		0.280		0.259		0.259	
Log-likelihood	-632.218		-576.711		-593.479		-593.479	

*** p<0.01, ** p<0.05, * p<0.1

Notes: In the first model Regchange - $_{(t-1)} \cdot Y_{(t-1)}$ is dropped and 10 observations not used, because != 0 predicts success perfectly. The asymptotic standard errors of the APEs were derived via the Delta Method (see Appendix B).

Table A.1 – Independent Variables, Description, Construction and Sources

Name	Description	Construction	Data Source
Log GDP/c _(t-1)	Logarithm of GDP per capita	$\ln(RGDPCH_{i,t-1})$	PWT 6.3
Inflation _(t-1)	Change in consumer prices	$100 \cdot \ln(1 + [gCPI_{i,t-1}])$	IFS & WDI ^a
Δ ToT _(t-1)	Change in terms of trade	$\ln(TOT_{i,t-1}) - \ln(TOT_{i,t-2})$	WDI & IFS ^b
Δ Real Exports _(t-1)	Change in exports volumes	$\ln(EXP_{i,t-1}) - \ln(EXP_{i,t-2})$	WDI & IFS ^c
RER Value _(t-1)	Real exchange rate valuation	see note ^d	PWT 6.3
Fin. Openness _(t-1)	Capital account openness	KAOPEN _{i,t-1}	Chinn-Ito '09
Trade Openness _(t-1)	Trade liberalization measure	1 if open in $t - 1$	W-W '08
Inequality _(t-1)	Gini coefficient for income	GINI _{i,t-1}	Solt '09
Polity2 _(t-1)	Revised combined polity score	DEMOC _{i,t-1} - AUTOC _{i,t-1}	Polity IV
Regchange + _(t-1)	Positive regime change	based on REGTRANS _{i,t-1} ^e	Polity IV
Regchange - _(t-1)	Negative regime change	based on REGTRANS _{i,t-1} ^e	Polity IV
Leader Exit _(t-1)	Irregular exit of leader	1 if EXIT _{i,t-1} $\neq 1$ ^g	Archigos 2.9
War/Conflict _(t-1)	Conflicts (≥ 1000 deaths)	see note ^f	UCDP/PRIO

^a We use the IFS series (CPI y-o-y %-change based on line 64) as a benchmark and append it with the WDI series in 59 cases where the former has missing data.

^b We use the WDI series as a benchmark (which comprises of UNCTAD and IFS data) and append it with the export volume index from the IFS for missing years/countries.

^c From the WDI 2011, we use the series ‘net barter terms of trade’, and from the IFS, we derive the equivalent net barter terms of trade by dividing the unit value of exports (line 74) by the unit value of imports (line 75) and multiplying the result by 100. We then append the WDI series of growth rates with the growth rates from the IFS series whenever the former has missing information.

^d [Rodrik \(2008\)](#) proposes a simple way to calculate an index of “real” exchange rate (RER) overvaluation based only on the Penn World Tables. The method involves three steps. (1) compute the PPP-adjusted exchange rate: $\ln(RER_{it}) = \ln(XRAT_{it}/PPP_{it})$. (2) Estimate the Balassa-Samuelson effect: $\ln(RER_{it}) = \alpha + \beta \ln(RGDPCH_{it}) + \gamma_t + \varepsilon_{it}$. (3) Take the difference between the actual RER and the predicted RER from (2), hence: $RER\ Value_{(i,t-1)} = \ln(RER_{(i,t-1)}) - \ln(\widehat{RER}_{i,t-1})$.

^e We use the Polity IV variable REGTRANS to identify regime changes in either direction based on a minimum 3-point change in a country’s democracy or autocracy score. We exclude the code 0 for “minor changes”, which denotes *any* change in the democracy or autocracy scores. Further, we do not code -77 for “interregna”, -66 for (foreign) “interruptions” and -88 for regime “transitions” as negative regime changes to avoid collinearity with the leader exit and war/conflict dummies.

^f This dummy is constructed based on the UCDP/PRIO Armed Conflict Dataset v.4-2011, 1946–2010. We first converted the conflict-year database into country-year format and then coded the intensity levels for the highest intensity conflict in any given country-year. The dummy is unity if the intensity level of the conflict was coded as 2 in $t - 1$ and the country was listed as a location of the conflict.

^g The Archigos 2.9 time-series database records entries, tenure and exits of country leaders and the conditions on which they entered and exited. In some instances there are multiple observations per country-year, in such an event we code an irregular exit if any one observation within that year is identified as “irregular”. Irregular exit refers to leaders that died in office, committed suicide, or left office due to ill health, other irregular means or the deposition by another state.

Appendix B

Partial effects in non-linear models

Contrary to linear models, partial effects in a non-linear model are not constant. Precisely because the relationship is non-linear, the derivative at any point depends on the value of the function at that point. For example:

$$\frac{\partial F(\mathbf{x}\boldsymbol{\beta})}{\partial x_k} = \frac{\partial F(\mathbf{x}\boldsymbol{\beta})}{\partial \mathbf{x}\boldsymbol{\beta}} \cdot \frac{\partial \mathbf{x}\boldsymbol{\beta}}{\partial x_k} = F'(\partial \mathbf{x}\boldsymbol{\beta})\beta_k \quad (1)$$

This relationship is most commonly summarized by the partial effect at the mean, partial effect at a representative value, or the average partial effect (APE). Increasingly, the APE is becoming the standard measure; it is defined as:

$$\text{APE}(\beta_k) = N^{-1} \sum_{i=1}^N F'(\partial \mathbf{x}\boldsymbol{\beta})\beta_k \quad (2)$$

The calculus method is not the most appropriate if $x_k \in \mathbf{x}$ is a dummy variable. In this case, a finite difference method for a change from zero to unity should be applied:

$$\text{APE}(\beta_k) = N^{-1} \sum_{i=1}^N [F(\mathbf{x}\boldsymbol{\beta}|x_k = 1) - F(\mathbf{x}\boldsymbol{\beta}|x_k = 0)] \quad (3)$$

However, as [Ai and Norton \(2003\)](#) pointed out, the coefficient of an interaction term in non-linear models does *not* reveal the *sign*, *size* and *significance* of the interaction effect. To see this, consider a simple binary model with one interaction term:

$$P(y_i = 1) = F(u) = F(\beta_0 + \beta_1 x_i + \beta_2 z_i + \beta_{12} x_i z_i) \quad (4)$$

In linear functions the partial effect is the coefficient β_{12} , but only because in the linear case β_{12} corresponds to the cross-partial derivative $\frac{\partial}{\partial z_i}(\frac{\partial F(u)}{\partial x_i}) = \frac{\partial^2 F(u)}{\partial x_i \partial z_i}$ of $F(u)$.

Applying the cross-partial derivative to the non-linear function $F(u)$ yields:

$$\frac{\partial^2 F(u)}{\partial x_i \partial z_i} = \frac{\partial[(\beta_1 + \beta_{12} z_i)F'(u)]}{\partial z_i} = \beta_{12}F'(u) + (\beta_1 + \beta_{12} z_i)(\beta_2 + \beta_{12} x_i)F''(u) \quad (5)$$

From (5) it is easy to see that β_{12} is not equal to the interaction effect. Instead, the true effect is given in (5) and often involves a numerical approximation for $F''(\cdot)$ if the interacted variables are both continuous.

The correct interpretation/estimation of interaction effects is still debated in the econometric literature. For example, [Puhani \(2012\)](#) shows that in ‘difference-in-difference’ models the cross-difference does not correspond to the treatment effect, but acknowledges that in many other applications cross-differences are in fact the parameters of interest. [Greene \(2010\)](#) illustrates that cross-derivatives include a functional form factor which makes them non-zero even in the absence of a significant interaction effect and suggests ways to graphically examine interactions. Nevertheless, [Greene \(2010\)](#) also notes that in the case of continuous by categorical interactions the difference in derivatives can be used to test the significance of a regime switch. In this application, we are precisely interested in differences in probabilities due to regime switches (in $y_{i,t-1}$).

Partial Effects: Fixed Effects Logit

The fixed-effects (conditional) logit estimator generally does not permit the computation of partial effects, as the unobserved heterogeneity (μ_i) is conditioned out of the estimated likelihood function. In our application, we showed that the results from the conditional logit and the unconditional logit with dummy fixed-effects are rather similar. Hence, we use the results of the dummy variables fixed effects logit model to approximate two types of partial effects: the individual partial effects and APEs. Since estimates of partial effects for non-interacted variables are straightforward to derive, we only illustrate how we estimate the partial effects and associated standard errors of interaction terms.

Consider a simplification of our basic dynamic model with one explanatory variable, the lagged state, an interaction term and fixed effects:

$$F(\beta_0 + x_{it}\beta_1 + \beta_2 y_{i,t-1} + x_{it}y_{i,t-1}\beta_{12} + \mu_i) \quad (6)$$

where $F(\cdot)$ is the cumulative logistic distribution: $F(u) = e^u / (1 + e^u)$.

If x_{it} is continuous and $y_{i,t-1}$ were continuous, then the partial effect of β_{12} is the cross derivative of $F(\cdot)$ with respect to x and $y_{i,t-1}$ as shown before. However, since $y_{i,t-1}$ is binary, the interaction effect is estimated using differences in partial derivatives:

$$\begin{aligned} \frac{\Delta(\partial F(u)/\partial x_{it})}{\Delta y_{i,t-1}} &= \frac{\Delta[(\beta_1 + \beta_{12}y_{i,t-1})F'(u)]}{\Delta y_{i,t-1}} \\ &= (\beta_1 + \beta_{12})F'(\beta_0 + \beta_1 x_{it} + \beta_2 + \beta_{12}x_{it} + \mu_i) - \beta_1 F'(\beta_0 + \beta_1 x_{it} + \mu_i) \end{aligned}$$

If x_{it} is also binary, we use double differences:

$$\begin{aligned} \frac{\Delta^2 F(u)}{\Delta x_{it} \Delta y_{i,t-1}} &= \frac{\Delta[F(u|x_{it}=1) - F(u|x_{it}=0)]}{\Delta y_{i,t-1}} \\ &= F(u|x_{it}=1, y_{i,t-1}=1) - F(u|x_{it}=1, y_{i,t-1}=0) \\ &\quad - F(u|x_{it}=0, y_{i,t-1}=1) + F(u|x_{it}=0, y_{i,t-1}=0) \end{aligned}$$

From these observation specific partial effects, we can easily compute the APE of the interaction term as follows:

$$APE(\beta_{12}) = N^{-1} \sum_{i=1}^N \frac{\Delta(\partial F(u)/\partial x_{it})}{\Delta y_{i,t-1}} \quad (7)$$

where ∂ denotes either partial derivatives or differences depending on if the corresponding variable x_{it} is discrete or continuous.

The standard errors of the APEs are derived using the delta method (Greene, 2011, pp. 696–699), which approximates the asymptotic variance as:

$$\widehat{\text{Var}}[APE(\widehat{\beta}_{12})] = \overline{\mathbf{G}} \hat{\mathbf{V}} [\widehat{\boldsymbol{\beta}}] \overline{\mathbf{G}}' \quad \text{where} \quad \overline{\mathbf{G}} = N^{-1} \sum_{i=1}^N \frac{\partial}{\partial \widehat{\boldsymbol{\beta}}'} \left(\frac{\Delta(\partial F(u|\boldsymbol{\beta} = \widehat{\boldsymbol{\beta}}))}{\partial x_{it} \Delta y_{i,t-1}} \right) \quad (8)$$

where $\hat{\mathbf{V}}$ is the estimated asymptotic covariance matrix of $\widehat{\boldsymbol{\beta}}$. For the individual partial effects, the Jacobian vectors are observation-specific and not averaged over N . This approach can easily be extended to account for more complex interactions as they appear in this paper, only the derivatives/differences require more computations.

Partial Effects: Dynamic Random Effects Probit

Wooldridge (2005) shows that in the dynamic random effects probit model, a consistent and \sqrt{N} -asymptotically normal estimator of the APEs of time-varying variables is available. The effects of time-invariant covariates are not identified. Using the same assumptions as in Section 5.3, we can write the expectation as:

$$E[\Phi(\mathbf{x}'_t\boldsymbol{\beta} + \rho y_{i,t-1} + \mu_i)] = E[\Phi(\mathbf{x}'_t\boldsymbol{\beta} + \rho y_{i,t-1} + \eta_0 + \eta_1 y_{i0} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_2 + u_i)] \quad (9)$$

where \mathbf{x}_t denotes time-varying regressors and $\bar{\mathbf{x}}_i$ their time-averages. The expectation runs over the distribution of $(y_{i0}, \bar{\mathbf{x}}_i, u_i)$, but we do not estimate parameters for μ_i or u_i .

Following Wooldridge (2005), we can get rid of the unobserved effects by applying the law of iterated expectations:

$$\begin{aligned} E[E[\Phi(\mathbf{x}'_t\boldsymbol{\beta} + \rho y_{i,t-1} + \eta_0 + \eta_1 y_{i0} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_2 + u_i) | y_{i0}, \bar{\mathbf{x}}_i]] = \\ E[\Phi[(\mathbf{x}'_t\boldsymbol{\beta} + \rho y_{i,t-1} + \eta_0 + \eta_1 y_{i0} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_2) \cdot (1 + \sigma_u^2)^{-1/2}]] \end{aligned} \quad (10)$$

Given the assumptions that $u_i \sim N(0, \sigma_u^2)$ and $e_{it} \sim N(0, 1)$, the scaled parameters in (10) are exactly the coefficients that standard random effects probit estimates; we can thus write their descaled counterparts more succinctly as $\Phi(\mathbf{x}'_t\boldsymbol{\beta}_u + \rho_u y_{i,t-1} + \eta_{u,0} + \eta_{u,1} y_{i0} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_{u,2})$. Putting these together, we have eliminated the group-specific unobserved effects and can rewrite (9) as:

$$E[\Phi(\mathbf{x}'_t\boldsymbol{\beta} + \rho y_{i,t-1} + \mu_i)] = E[\Phi(\mathbf{x}'_t\boldsymbol{\beta}_u + \rho_u y_{i,t-1} + \eta_{u,0} + \eta_{u,1} y_{i0} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_{u,2})] \quad (11)$$

A consistent estimator of this expectation is the simple average across all observations ($N^{-1} \sum_{i=1}^N \dots$). The derivative of this function with respect to a continuous time-varying regressor, or the finite difference for a binary regressor, is equivalent to the APE of that variable. This approach can be extended to include interactions with the lagged state and other non-linearities. Wooldridge's device to get to the APEs is to always average across the distribution of $(y_{i0}, \bar{\mathbf{x}}_i)$ first and then to specify the derivatives/differences. For example, the APE of a continuous variable (without an interaction term) in \mathbf{x}_t is:

$$\text{APE}(\beta_k) = N^{-1} \sum_{i=1}^N \phi(\mathbf{x}'_t\boldsymbol{\beta}_u + \rho_u y_{i,t-1} + \eta_{u,0} + \eta_{u,1} y_{i0} + \bar{\mathbf{x}}'_i\boldsymbol{\eta}_{u,2}) \beta_{u,k} \quad (12)$$

where ϕ is the probability density function of the standard normal cdf.

The APEs of interaction terms are derived using double differences or differences in partial derivatives. In all cases, the standard errors are computed using the delta method as in defined in (8) of the previous section. We specify analytic differences in derivatives or double differences for the interaction effects, while the observation specific Jacobian vectors with respect to the coefficient-vector are computed numerically.

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